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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1868, *TO JUNE* 1869.

VOL. XXIX.

BEING THE ANNUAL HALF-VOLUME OF THE MEMOIRS AND PROCEEDINGS OF THE ROYAL ASTRONOMICAL SOCIETY.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *November* 13, 1868. No. 1.

Admiral MANNERS, President, in the Chair.

Sir William Thomson, Glasgow University;
Rev. E. Crofton, Oak Lawn, Wootton Bridge, Isle of Wight;

were balloted for and duly elected Fellows of the Society.

The Society have received from the Hydrographic Department of the Admiralty Observations of the Total Eclipse of 18th August, 1868, made by Staff-Commander Reed, commanding H. M. Surveying Vessel *Rifleman*, and Mr. Pope Hennessey, Governor of Labuan, at Barram Point, on the N.E. Coast of Borneo. Mr. Pope Hennessey's Observations have been published in the Proceedings of the Royal Society.

The Society have also received from the Peninsular and Oriental Company accounts of the Eclipse, as seen by their Commanders — Captain H. W. King, Latitude 16° 44′ N., Longitude 83° 55′ E., within the totality — Captain C. G. Perrins, commanding S. S. *Carnatic*, Latitude 16° N., Longitude 54° 15′ E., 20 miles north of the limit of totality on that meridian — Captain D. Rennoldson, commanding S. S. *Rangoon*, Latitude 15° 42′ N., Longitude 59° 15′ E., on the central line — and Captain J. S. Murray, then in the Formosa Channel, considerably north of the limit of totality.

Observations of the Solar Eclipse, August 18th, 1868.

By J. Tebbutt, Esq.

The solar eclipse of the 18th Aug. was pretty well observed here. Although the Sun was occasionally hidden from view by passing clouds, the beginning and the greatest obscuration were well seen. A remarkable phenomenon presented itself about half a minute before the instant of first contact. A dark streak or band became visible for several degrees along that limb of the Sun to which the Moon was approaching, and after continuing visible for a few seconds, vanished. It reappeared, however, for a moment immediately before the contact. The darkness seemed to extend along the narrow interval separating the limbs of the Sun and Moon, but did not encroach on the solar disk. This phenomenon was so palpable that it served as an excellent warning of the time of contact. A very slight flattening of the Sun's limb was also remarked just previous to the contact, but this was quite distinct from the indentation produced by the Moon. The Sun was shining beautifully clear at the time. The first contact was pretty accurately observed at $4^h\ 6^m\ 30^s$, local mean time, the telescope employed being my $3\frac{1}{4}$-in. refractor with a power of about 30. During the progress of the eclipse there was a slight boiling of the limb both of the Sun and Moon. This, however, was the result of atmospheric causes, and was not so great as I expected from the low altitude of the objects. With the exception of the slight disturbances from this cause, the Sun's cusps were sharp and well defined. A careful scrutiny satisfied me that not the slightest portion of the Moon's limb could be even faintly distinguished beyond the cusps. No remarkable irregularities were observed on the Moon's limb. Two very fine spots, each surrounded with an extensive penumbra, and attended with several smaller spots, were very conspicuous on the southern half of the Sun's disk, but were not eclipsed by the Moon. A magnificent tract of faculæ, situated near the larger spot, contrasted very beautifully in colour with the general ground of the solar disk. About one-third of the Sun's diameter was eclipsed at the time of greatest obscuration. The Sun disappeared behind a dense cloud on the horizon at $5^h\ 16^m$ P.M.

Meteorological observations were taken through the day, but nothing remarkable, beyond the unusual steadiness of the barometer, was recorded. The observations of the black-bulb thermometers were much interfered with by passing clouds.

Windsor, New South Wales,
September 1st, 1868.

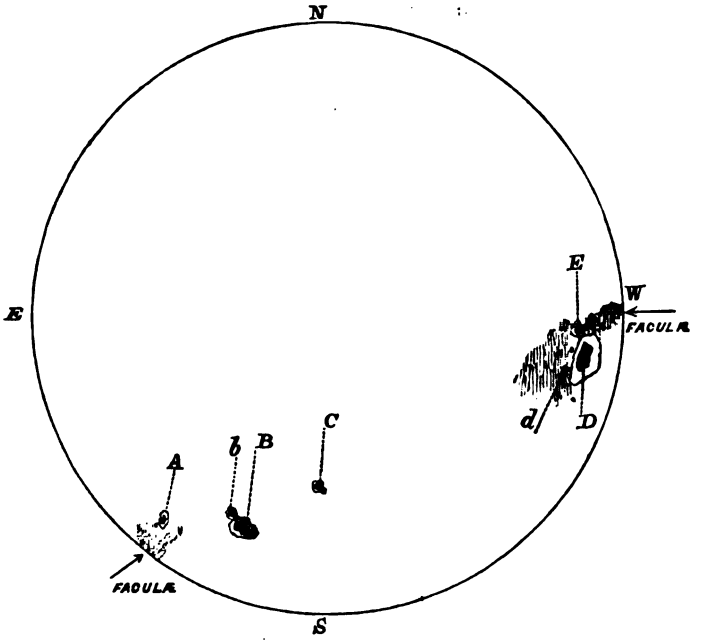
Sun-spots and general Aspect of the Sun on the day of the Total Eclipse, August 18th, 1868.

(*Communicated by Messrs. De la Rue, Stewart, and Loewy.*)

The history of the great eclipse would, in our opinion, be incomplete without a record of the heliographical positions of the Sun-spots observed at the time.

The unfavourable weather in England on the days before and after the eclipse prevented photographic observations, but we were so fortunate as to obtain a sun-picture on the day of the eclipse, at $3^h\ 29^m$ G.M.T., about eight hours after the phenomenon.

There were two comparatively large groups of spots on the Sun, one near the eastern, the other near the western, limb. Both groups were connected, as shown in the accompanying rough



Rough Sketch of Aspect of Sun.

sketch, with faculous matter which apparently extended beyond the visible disk on both sides. The faculæ consisted chiefly of very numerous small scattered patches, here and there running together and forming larger flocculi; they covered altogether an unusually large area, indicating the extraordinary activity which

was also observed by Dr. Janssen, during and after the time of the eclipse, on the surface of the Sun.

The angles of position of those two parts, where the faculæ seemed to extend into the invisible hemisphere, may roughly be stated as 145° for one patch, and 255° for the other, reckoning from north towards east. How far these positions agree with any observed protuberances can only be gathered after some time, when the whole of the detailed accounts have come to hand.

The heliographical elements of the Sun-spots, forming the two groups, were calculated in accordance with the principles explained in a paper read before the Royal Society some time ago, which we hope to see soon in the hands of the printer. These elements are given in the following table, where the letters for each spot correspond with those on the accompanying sketch.

Heliographical Elements of the Sun-spots, observed at the Kew Observatory, on August 18, 1868, $3^h\ 29^m$ *G.M.T.*

Spots.	Scale-Reading.	Dist. from Centre.	Dist. in terms of Radius.	Circle-Reading.	Angle of Position.	Heliographical Lat.	Heliographical Long.
				° ′	° ′	° ′	° ′
A	0·287	1·729	0·908	135 18·0	135 3·8	−21 34	191 51·2
B	0·564	1·452	0·763	147 3·2	146 49·0	−23 37	211 40·0
b	0·563	1·453	0·763	145 22·5	145 8·3	−22 34	210 52·7
C	0·810	1·206	0·633	158 38·3	158 24·1	−23 10	225 57·3
D	0·271	1·745	0·917	251 52·6	251 38·4	−28 55	309 30·8
d	0·317	1·699	0·892	250 23·3	250 9·1	−28 47	305 29·1
E	0·303	1·713	0·900	254 3·1	253 48·9	−26 12	307 59·4

On a possible Method of viewing the Red Flames without an Eclipse. By William Huggins, F.R.S.

In the Report of my Observatory at the last Anniversary (p. 88 of the last volume), it is stated that "during the last two years numerous observations have been made for the purpose of obtaining a view of the red prominences seen during a solar eclipse. If these bodies are gaseous their spectra would consist of bright lines. With a powerful spectroscope the light reflected from our atmosphere near the Sun's edge would be greatly reduced in intensity by the dispersion of the prisms, while the bright lines of the prominences, if such be present, would remain but little diminished in brilliancy. This principle has been carried out by various forms of prismatic apparatus, and also by other contrivances, but hitherto without success." The observations of the eclipse of August last having shown the position in the spectrum of the bright lines of the red flames, Mr. Lockyer and Mr. Janssen succeeded independently, by a similar method, in viewing the spectra of these objects.

My object in this Note is to describe one of the "other contrivances" mentioned in the Report.

The apparatus consisted of screens of coloured glasses and other absorptive media, by which I was able to isolate portions of the spectrum. It appeared highly probable that if the parts of the spectrum which then alone remained were identical with those in which the bright lines of the flames occur, these objects would become visible.

For this inquiry I obtained a great variety of coloured glasses and other absorptive media. I first examined them with a prism to learn the absorptive power which they exercised on different parts of the spectrum. I then combined them in various ways. These glasses were sometimes employed before the eye, but more frequently by projecting the image of the Sun's edge upon a screen, after the light had been sifted by the coloured media. In making these experiments means were taken that the whole of the Sun's image should be got rid of, in order that the eye, kept in comparative darkness, might be more sensitive to the greatly feebler illumination of the objects sought for. As I had no knowledge of the position in the spectrum of the bright lines, it would have been by accident only if I had succeeded in obtaining a view of the flames.

Now that the positions of these lines are known, this method appears to be very promising. Perhaps the light about the red line at C will be most easily isolated. I have a deep ruby glass which cuts off all the spectrum except the extreme red. I have since the observations only been able to make one attempt, when the state of the atmosphere was unfavourable.

It is obvious that by this method the form and appearance of these flames could be observed, and the objects measured with accuracy.

On a possible Method of viewing the Red Flames without an Eclipse.

(*Extract from Letter from Sir John Herschel, Bart. to the Rev. C. Pritchard.*)

I send you an extract from a letter of my son, Lieut. Herschel, from which you will see that immediately on his ascertaining the fact that the light of the protuberances exhibited three *bright* lines corresponding to *dark* lines in the light of the Sun's photosphere, he was at once led to speculate on the possibility of taking advantage of this fact to obtain a view of the protuberances apart from the condition of a total eclipse. That such *glasses* as he contemplates (or other absorptive media) could be found, there is, I fear, little hope; and my only view in calling attention to this suggestion is to show how immediately and readily a clearly defined new fact of this sort suggests to an active and combining intellect the possibility of immediate practical application. If I mistake not, the "double discovery" of Mr. Lockyer and Mr. Janssen, of a mode of rendering the pro-

tuberances *sensible* (not visible) by spectroscopic analysis of the light close to, *but beyond* the photosphere in ordinary daylight, turns upon the very same feature — viz. the *absence* of the three rays D, &c. in the photospheric light (and therefore in the *general* skylight) and its *presence* in that portion of the skylight which is superposed on the prominences; so that, while skylight *in general* exhibits necessarily three *black* lines corresponding to D, &c. — at that particular spot where a prominence exists, they should not be exhibited at all, or very feebly.

Extract of a Letter from Lieut. J. Herschel, dated Bangalore, Sept. 2, 1868.

"It has occurred to me that, since the whole light of the 'flames' is of three refrangibilities only (or nearly so), dark glasses (could they be formed) which allowed these *only* to pass, would so enormously diminish the light *from the Solar disk*, as to enable the flames to be seen without the interposition of an opaque body, *i.e.* without an eclipse. When I say dark glasses, I mean, of course, *any* media which have the effect required. Is the idea a practical one? If it is, these flames may come to be studied *at leisure!*

"I will not forget *Argûs* you may depend."

Collingwood, Nov. 3, 1868.

Remarks on Mr. Stone's Rediscussion of the Transit of Venus, 1769. By S. Newcomb, Esq.

I beg leave to offer to the Royal Astronomical Society the following remarks on Mr. Stone's very interesting re-discussion of the observations of the transit of *Venus* in 1769, which appears in the October number of the *Monthly Notices*.

Mr. Stone's interpretation of Chappe's observation of first contact at egress seems to me untenable. To understand the question at issue it must be borne in mind that, owing to irradiation, after the planet has apparently entered upon the solar disk, it is still connected with the latter by a dark protuberance, or "black drop," which rapidly grows thinner, and at last breaks off, leaving the planet at a sensible distance from the edge of the disk. This phenomenon marks the true time of external contact, and is that to which the attention of the observer should always be directed. At the second external contact the same phenomenon may be observed in a reverse order, a black ligament suddenly forming between the planet and the edge of the solar disk.

The question is whether, at this contact, Chappe observed the sudden formation of the ligament, or whether he waited for the moment when the apparent limbs of the two bodies would have touched each other if the drop had not been there. Chappe says (I quote from Mr. Stone):

"Temps vrai $5^h\ 54^m\ 50^s$. Premier contact à la sortie. * * * A ce premier contact *Vénus* s'est allongée plus considérablement que le matin en s'approchant tout-à-coup du bord du Soleil."

If, as Mr. Stone assumes, the time given in the first sentence of this extract does not refer to the phenomenon described in the last sentence, it must be either (1) that by the words "ce premier contact" Chappe meant something different from the "premier contact" he had observed, or that by the preposition "à" he meant some time before. His language does not seem to me to admit of either of these meanings. Moreover, from his account of the ingress it appears that Chappe was acquainted with the phenomena, and knew what was to be observed. "Je n'observai point pour l'instant de l'entrée total celui ou le bord de *Vénus* commençoit à s'allonger, mais ne pouvant pas douter que ce point noir ne fît partie du corps opaque de *Vénus*, j'observai le moment où il étoit à sa fin."

Mr. Stone's view does not seem at all strengthened by the observation of Pauly, for Chappe says he saw *Venus* perfectly after Pauly had left his telescope, and intimates that the reason of his leaving so soon was his want of practice in observation. But if Mr. Stone's view is right, and if Chappe saw the phenomenon he describes, Chappe must have seen the very thing he wanted to observe at the moment Pauly left the telescope. His last remark would then hardly be consistent with honesty.

A re-solution of Mr. Stone's equations on the hypothesis here maintained leads to the following results:—

Normal Equations.

$$16x - 64{\cdot}4\,\delta\varpi = +358{\cdot}0$$
$$21y - 110{\cdot}2\,\delta\varpi = -56{\cdot}4$$
$$-64{\cdot}4\,x - 110{\cdot}2\,y + 123134\,\delta\varpi = +45536.$$

Solution.

$$x = +24^{s}{\cdot}0 \qquad y = -0^{s}{\cdot}6$$
$$\delta\varpi = +0''{\cdot}38 \qquad \varpi = 8''{\cdot}87$$

If the equations in x and y are treated separately, the results will be—

From equations in x, $\varpi = 8''{\cdot}91$
From equations in y, $\varpi = 8''{\cdot}70$.

A Reply to Mr. Newcomb's Remarks on "Mr. Stone's Rediscussion of the Transit of Venus, 1769." By E. J. Stone, Esq.

I would remark, in the first place, that Mr. Newcomb has not touched any point of primary importance in this discussion. The real issue between the old value, of 8″·58, and mine, of 8″·91, is no less than 0″·33. The point Mr. Newcomb has raised is a question of only 0″·04, viz. between my value and 8″·87. The question is, therefore, one of comparative insignificance, but the observations will not, I believe, bear even this slight change.

Mr. Newcomb's point is, that I have erroneously assumed that Chappe, at the egress, observed an apparent internal contact instead of the real one or formation of the black drop. His only argument is that it would be absurd to suppose that he would do so. I should have thought that my meaning was sufficiently clearly expressed. My words were, "I consider, from the description given by Chappe himself, that at the egress he did not catch the formation of the black drop. He appears to have been surprised by the contact being established all at once." Take these words in conjunction with Chappe's description of his observation, "Le soleil étoit ondoyant ainsi que *Vénus*, ce qui rendoit cette observation très difficile. A ce premier contact *Venus* s'est alongée plus considérablement que le matin, en s'approchant tout-à-coup du bord du Soleil." It appears to me you have exactly my meaning, that although Chappe fully intended to observe the same phenomenon at the egress which he observed at the ingress, yet owing to tremor and the difficulty of the observation he did not catch the first formation of the drop, but some phase much nearer to the apparent internal contact. Calling to mind Wales' observation of 24ˢ between the real and apparent contacts at the egress, I maintain, in spite of Mr. Newcomb's remarks, that Pauly's positive observation of same contact 22ˢ before the time given by Chappe does strengthen most materially the inference which I have drawn from Chappe's own words.

It is an unfortunate fact for Mr Newcomb's argument about the "premier contact," that at the ingress Chappe describes the phenomenon observed not as a "contact" but as "l'entreé totale." Mr. Newcomb appears to me to have mixed up his own inferences and Chappe's words respecting Pauly and his observation. Pauly appears to have occupied a position equivalent to that of an English Royal Engineer. Chappe says that he was an observer of a little, *i.e.* some experience. An entirely different impression from that given by Mr. Newcomb's words.

Again, when Mr. Newcomb says that "Mr. Stone's view does not seem at all strengthened by the observation of Pauly, for Chappe says he saw *Venus* perfectly after Pauly had left his telescope," Mr. Newcomb has overlooked the fact that this disappearance of *Venus* refers to the *total egress* of the planet from

the disk, and therefore proves nothing with respect to Pauly's observation of internal contact about 19^m before.

The observations appear to have been arranged as follows: An assistant was stationed near the clock to give out the time, the observers themselves only comparing their time with the clock directly as a check. There is no doubt but that Chappe, with his more powerful instrument and the extraordinary care and precautions he took to catch the last glimpse of the planet on the Sun, did see *Venus* long after Pauly had lost sight of the planet. Chappe distinctly states that without the precautions he adopted to secure this last glimpse an enormous error would have been made. This, however, does not prove that Pauly's positive evidence of a contact, an entirely different and much easier observation, should be entitled to *no weight*. Chappe undoubtedly thought that Pauly's observation was too early, because he had not himself caught it. I consider this accounted for by the difficulty of the observation owing to tremor.

I am quite at a loss to find what evidence Mr. Newcomb has collected that Pauly left the telescope after the internal contact at egress, and that Chappe, his attention being thus called to the point, looked particularly and could then see no trace of the contact being established. There is no such statement in Chappe's journal, and Chappe never lived to return. Mr. Newcomb has blended together Chappe's remarks relating to the total egress, or final disappearance of the planet from the Sun's limb, and applied them by construction to the observation of internal contacts. If Chappe has made no remarks about Pauly leaving the telescope near the time of internal contact, Mr. Newcomb's dilemma has no force. I have, perhaps, dwelt too long upon this point, but it is the only one raised by Mr. Newcomb. There is a difficulty respecting these St. Joseph observations which has apparently escaped Mr. Newcomb's notice. There were three observers near St. Joseph, Chappe, V. Doz, and Medina. The observed durations agree so closely that whatever assumption be made respecting the one must be made respecting all. It certainly appeared to me so arbitrary an assumption that all three observers had missed the first formation of the black drop at the egress that, in spite of Chappe's language and Pauly's observation, I at first assumed that real contacts must have been observed. The value of solar parallax deduced from the equations on these suppositions was perfectly satisfactory to me, but the residual far exceeded any probable errors and the difference in time between the two phases was not satisfactory. I then, on re-consideration, came to the conclusion, which I still think satisfactory, that if Chappe's words indicate that he lost the first appearance of the black drop, the same atmospheric disturbances, the same boiling of the limbs of the Sun and *Venus*, similar difficulties of observation, in fact, might equally prevent the first appearance being caught by V. Doz and Medina, who were observing near. My argument is this,—We have two distinct phases of a pheno-

menon, the real and apparent contacts, separated roughly by about 20ˢ: a quantity too large to be confounded with mere error of observation. If an observer states positively that he has observed a real contact, I have no right to assume that he has observed an apparent contact. If the observer says that he has observed an apparent contact, I have no right to assume that he has observed a real contact; but if the observer's words are doubtful, and I find that, by assuming he has observed an apparent contact, all the observations are rendered accordant, whilst by assuming that he has observed a real contact, all the observations are rendered discordant, then I maintain that it is not only permissible, but necessary, that we should assume that apparent contacts were observed.

The appeal in such a case must be to the residual errors. I have here placed my argument on its lowest ground. I believe that Chappe's own words require the assumption which I have made.

The following are the residual errors in my solution and in the modified form proposed by Mr. Newcomb.

Observer.	Stone's *solution* s	Newcomb's *modification*	Mean for S.	Mean for N.
Hell	− 1·7	− 0·8		
Sajnovics	+ 2·8	− 5·3	+ 0·6	− 3·1
Wales	+ 0·4	+ 7·9		
Dymond	− 0·6	+ 8·9	− 0·1	+ 8·4
Rumovsky	− 0·9	+ 6·9	− 0·9	+ 6·9
Chappe	+ 0·5	− 6·6		
V. Doz	− 0·4	− 5·7	− 1·6	− 4·3
Medina	− 5·4	− 0·7		
Green	+ 5·8	− 1·8		
Cook	− 4·2	+ 8·2	+ 0·8	+ 3·2

Mr. Newcomb's modification has increased the sums of the squares of the residuals at the five stations nearly 40 times.

It was these residuals, the unsatisfactory value of $\frac{x-y}{2}$, and Chappe's own language concerning the difficulty of the observation at egress, the establishment of the contact all at once, and Pauly's positive observation of a contact 22ˢ before Chappe's observation, that drove me to assume that Chappe did not, as a matter of fact, catch the first formation of the drop, but some phase much nearer the apparent contact. Mr. Newcomb has only reproduced one of the many slight variations I have tried upon this question.

The following table will perhaps show how well this transit of *Venus* of 1769 was observed; how favourable, on the whole,

were the positions of the observers; how impassable a barrier any satisfactory discussion of these ten observed durations places between such values of the solar parallax as 8″·58 and 8″·91.

If the value 8″·58 of the solar parallax satisfied the ten observed durations, we should have to assume the following errors to have been made in the mean of the observed durations at each station, in order that 8″·91 could be admitted as the true value of the solar parallax.

+ means that the observed duration is too small.

Station.	No. of Obs. durs.	Greatest discordance.	Assumed Mean Error.	Stone's Residuals.
		s	s	s
Wardhus	2	4·5	+24·9	+0·6
Kola	1	..	+25·3	−0·9
Hudson's Bay	2	1·0	+ 7·8	−0·1
St. Joseph	3	5·9	−10·3	−1·6
Otaheite	2	10·0	−27·5	+0·8

Such errors, as the results of observation, are simply impossible, and if a satisfactory solution of these ten durations had ever been obtained with a value of the solar parallax 8″·58, then, whatever might have been the results derived from our other methods, the true value of the solar parallax could not have been 8″·91, nor any value near it. The assumed mean errors show most clearly the grounds upon which astronomers refused to give up the value 8″·58. The error was impossible, only provided the discussion of the results had been satisfactory. The solutions obtained, however, with the value 8″·58 have never satisfied the observed durations. Exaggerating the importance of some alterations in Hell's Journal, a solution has been forced by virtually assuming the observations at Wardhus to have been forgeries. Even this, however, has not been sufficient, the Kola observations still stood out as a warning. In my solution I have rejected nothing for discordance. The residuals are all reduced, in the mean results, within 1ˢ·6, a quantity within probable errors of observation. I consider, therefore, that this great problem of the determination of the mean distance of the Sun from the Earth, may be considered as almost settled within the requirements of our present astronomy. All our work is now accordant, and points to a value about ninety-one millions seven hundred thousand miles.

Observations made at the Royal Observatory, Greenwich, of the Egress of the Planet Mercury from the Sun's Disk, 1868, *Nov.* 4.

(*Communicated by the Astronomer Royal.*)

Observations by E. J. Stone.

The instrument employed was the Great Equatoreal. Aperture of object-glass, 12¼ inches.

My attention was chiefly directed to any peculiarities which might be presented during the transit, and not to time-determinations.

With power 137, a ring of light was clearly visible around the disk of *Mercury*. It extended to a distance of nearly a semi-diameter.

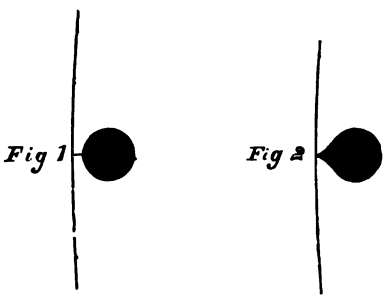
The ring was still visible with a power of 245, and did not disappear with any changes in the coloured glasses employed. I am of opinion that it arose from mere contrast.

The central part of the disk was certainly blacker than the edges.

I could see no central bright spot.

I could see no satellite.

At the first contact, without the slightest deviation from circular form, a very fine ligament was suddenly seen, connecting the planet and the Sun's apparent limb (fig. 1). This ligament



very quickly became broader, and made it extremely difficult to estimate the time of apparent internal contact, which, however, I estimated at about fifteen seconds after the first phenomenon of the connecting ligament. On looking at the half-second chronometer after the formation of the ligament, the transit-clock time was $11^h\ 59^m\ 14^s{\cdot}5$, but this was some seconds after the first formation of the ligament. The corresponding G.M.T. would be $21^h\ 0^m\ 5^s{\cdot}5$.

Observations by Edwin Dunkin.

The observations were made with the Altazimuth; power about 100; clear aperture of object-glass $3\frac{3}{4}$ inches:

	h	m	s	
First contact	21	0	6·2	G.M.T.
Last contact	22	2	35·7	

The time recorded at the first contact was when the limb of the planet was first seen in coincidence with the Sun's limb, at the moment when it was noticed suddenly to assume a balloon or pear shape. I could not see any very narrow filament between

Mercury and the Sun's limb before the recorded time. The apparent internal contact of the limbs must have been about 10ˢ after the recorded time. At the first contact the appearance was similar to that represented in Fig. 2.

Mercury was densely black in the interior parts of the planet, but near the limbs I thought that there were appearances of a slight illumination. The limbs had a hazy appearance. No bright spots seen on *Mercury*. The observed times were registered on the chronograph.

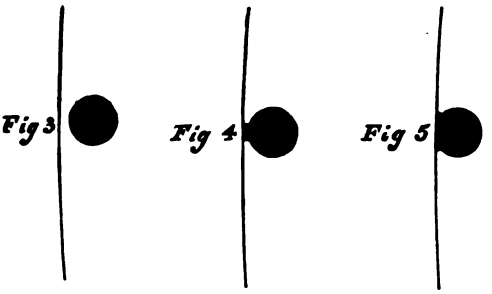
Observations by G. S. Criswick.

The observations were made near the thermometer stand on the magnetic ground, with the 46-inch telescope, and power of 170. The times were observed with a mean solar chronometer (Carter 154) which was compared by means of coincidences with the transit-clock directly afterwards.

	h	m	s	
First contact	21	0	12·8	G.M.T.
Last contact	21	2	42·8	

The limb of *Mercury* seemed very rough when seen on the Sun's disk.

Just before the planet arrived at the Sun's limb, it became somewhat elliptical (fig. 3), and then suddenly sent out a black



point to the Sun's limb (fig. 4): this was the phenomenon observed for "first contact:" the time given is perhaps two or three seconds late, as I was not counting the chronometer at the time. The observation of the last contact is considered pretty accurate.

The distortion of *Mercury* made the time of "bisection" too uncertain to be worth observing.

Observations by W. T. Lynn.

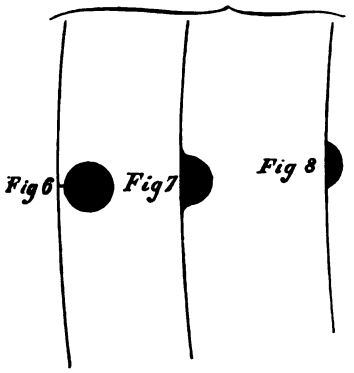
The observations were made with the North Equatoreal, power about 170.

	h	m	s	
First contact of filament	21	0	0·7	G.M.T.
Contact of limbs established by	21	0	32·6	
Bisection	21	1	31·5	
Last contact	21	2	37·3	

The planet as it approached the Sun's limb appeared, as it were, elliptical on the side nearer the Sun's centre.

The first time noted is when a thread or filament of blackness seemed to issue from the planet and touch the Sun's limb (fig. 6). It is considered pretty exact.

The time of contact of limbs is doubtful, the planet assuming the shape shown in fig. 7. The time is, therefore, an estimation that they were certainly in contact by at least that time.



The time of bisection is probably true to within about 2 or 3 seconds (see fig. 8).

The last contact is, I believe, true within a second, as I certainly saw an indication of the planet a second before the time noted, at which time it had totally disappeared.

The Sun's limb was well defined, and the tremor by no means considerable.

Observations by J. Carpenter.

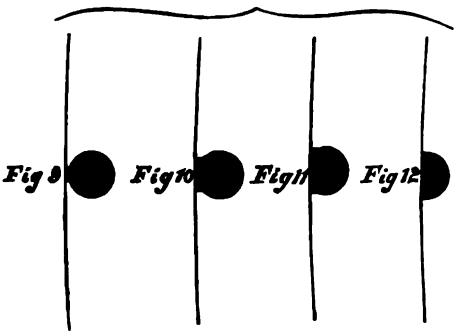
The telescope used was one of the former collimators of the Transit Circle; 3¾-in. aperture, 62-in. focus, power 90.

	h	m	s	
First contact	21	0	14·1	G.M.T.
Last contact	21	2	40·1	

At the first contact the disk of *Mercury* was distorted as shown in fig. 9.

The black ligament appeared to form instantly and to be of the breadth indicated, about ¼ the diameter of the planet.

The same character of distortion remained till the planet crossed the edge of the Sun; the successive phases being as shown in figs. 10, 11, 12.



The squareness of outline of the black ligament was very striking; its appearance was not altered by varying the density of the glass shade. A graduated shade was used.

Observations by H. J. Carpenter.

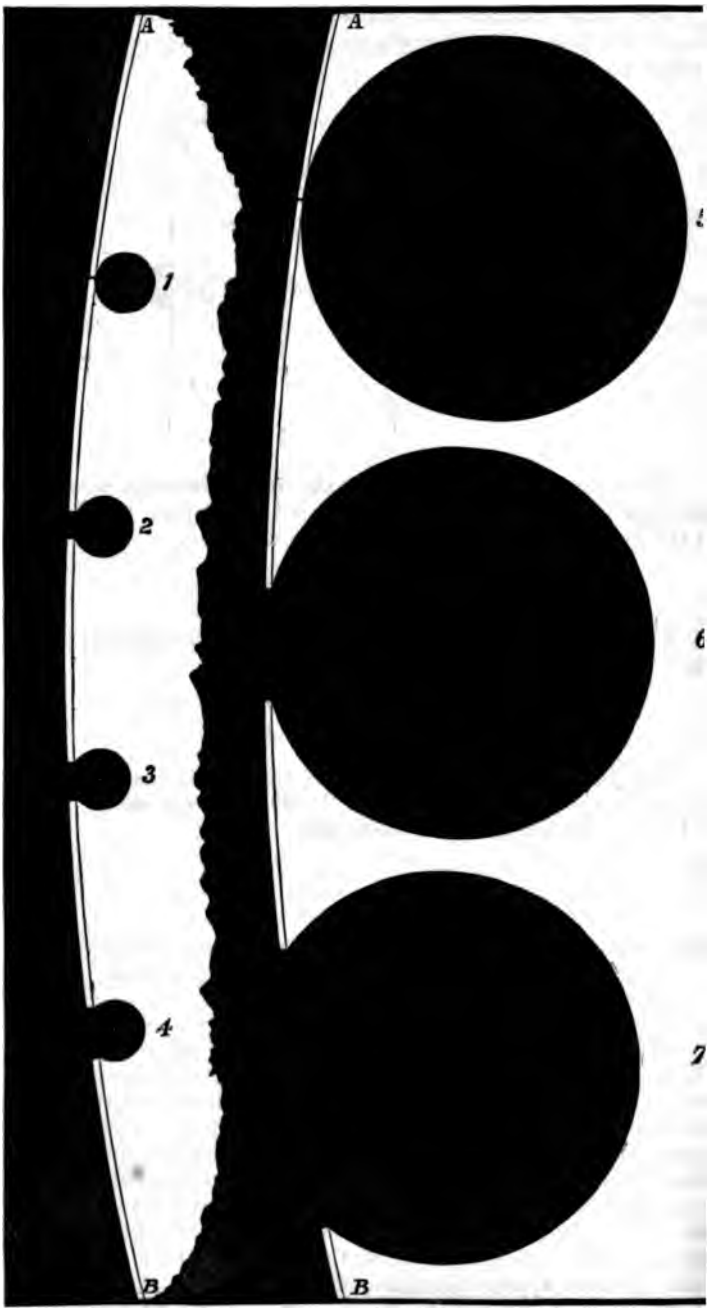
The observations were made with the East Equatoreal; power 70.

	h	m	s	
First contact	12	0	11·1	G.M.T.
Last contact	12	2	36·7	

The time noted for first contact is that of internal contact of the limbs when light first ceased to be seen between them. That of last contact is considered pretty exact.

Some Remarks and Suggestions arising from the Observations of the Transit of Mercury across the Sun's Disk, Nov. 4, 1868. By E. J. Stone, Esq.

The diagram with this paper is intended to represent the phenomena presented at the ingress and egress of *Mercury* and *Venus* across the Sun's disk. I have allowed about $\frac{1}{60}$th part of the diameter of *Venus* for the effects of irradiation. The extreme boundary of light and shade represents the apparent limb of the Sun. The true limb of the Sun may be considered to fall half way between A B, and the boundary of light and shade. When the true limb of the planet reaches the true limb of the Sun, or the apparent limb reaches A B, the light is cut off, and a fine dark ligament appears to connect the limbs of the planet and the


A
A
1
2
3
4
B
B

Sun. This is the true contact: the time of the formation or breaking of the so-called black drop.

Figs. 1 and 5 represent this phase respectively for *Mercury* and *Venus*.

Figs. 3 and 6. The appearances presented at apparent internal contact.

Figs. 2, 4, and 7. Appearances at intermediate phases.

Fig. (7) is intended to represent the phase of figs. 2 and 4 of Cook's and Green's observations in 1769, the point of comparison being the breadth of the ligament.

With respect to the Greenwich observers, the first appearance of the contact appeared to me to take place without the slightest previous distortion of the form of *Mercury*, the ligament appearing almost like a cobweb for fineness.

Mr. Lynn appears to have first caught a phase very near that of fig. (1). Mr. Dunkin, a phase between figs. (2) and (3). Messrs. Criswick, James Carpenter, and H. Carpenter, phases somewhere between (2) and (3).

Mr. H. Carpenter has not given any drawing. He stated, however, that he tried to catch the time when the thread of light entirely disappeared. On my requesting him to point out which of the figures appeared to him to represent most nearly the appearance of the planet at the time of his observation, he, without hesitation, pointed out fig. (3). His time agrees closely with the time given by Messrs. Criswick and James Carpenter. I would particularly call attention to the differences in aperture and optical power of the instruments employed by the different observers.

It appeared to all of the observers difficult to catch the time of apparent internal contact; figs. (3) and (6) are intended to show the comparative facility with which this phase can be seized in the case of *Venus*.

As practical hints for guidance in future observations of a Transit of *Venus*, I would suggest for consideration the following points:

1. That telescopes of nearly the same aperture should be employed.
2. That very nearly the same magnifying powers should be employed by all the observers.

(3.) That attention should be directed to observations of real internal contacts as the chief points.

Phase Fig. (5).

(4.) That apparent internal contacts should also be most carefully observed.

Phase Fig. (6).

5. That in observing apparent internal contacts two times should be given corresponding to the "Contactus

dubius" and "Contactus certus" of Hell and Sajnovics, and the mean taken for the time apparent contact.

6. That diagrams or drawings should in all cases be furnished by the observers to represent, as nearly as possible, the appearance of the planet corresponding to each recorded time of observation. In making such drawings the essential point should be to represent accurately the comparative breadths of the ligament with respect to the diameter of *Venus*. It must be remembered that such words as "instantaneous" refer only to the observer.

I lay more stress upon the observation of apparent internal contacts, because I believe that under unfavourable atmospheric circumstances, the real internal contacts would not be generally caught by the observers until long after the contact had been established.

Observation of Transit of Mercury, end of, on November 4, 1868. By Professor C. Piazzi Smyth.

Telescope aperture = 6·4 in.; magnifying power = 80.

Definition very bad, from undulations in the atmosphere sometimes so excessive as to cause the planet, otherwise signally black and round, almost to disappear; and making its circumference and the Sun's limb always to consist, more or less, of broken ripplings of light and shade, completely preventing thereby any real phenomenon of the expected "*rupture of the luminous thread*" at internal contact.

	G.M.T. h	m	s
Internal contact, thus unfavourably observed, at	21	0	7
External contact, or planet completely off the Sun's disk, at ..	21	2	20

Royal Observatory, Edinburgh,
November 12, 1868.

The Transit of Mercury, November 4, 1868, observed at the Observatory, Durham. By J. J. Plummer.

(*Communicated by Prof. Chevallier.*)

The transit of *Mercury* was observed at Durham under exceptionally favourable circumstances; the Sun rose in a perfectly cloudless sky, and not a cloud was seen during the whole course of the observations. The limb of the Sun was, however, very

tremulous, and the planet ill defined, owing to the low altitude; but both improved very much towards the time of egress.

The instrument used was the Fraunhofer Equatoreal of the Observatory. Aperture, 6½ inches, and 8 feet 3 inches focal length, furnished with a wire-micrometer; power 112.

The times of the internal and external contacts at the egress were observed as follows:—

	Durham S.T.	Green. M.T.
	h m s	h m s
Internal Contact	11 53 21·0	21 0 12·0
External Contact	11 55 38·5	21 2 29·2

The first time is considered quite exact; the second, owing to the undulations of the Sun's limb, is less certain.

Some measures were made of the differences of right ascension of the Sun and planet; they have been corrected for refraction, parallax, and for the motion of the Sun; and are compared with the Elements given in the *Nautical Almanac.*

Durham Sid. T.	Green. M.T.	No. of Wires obser^{d}.	Limb of ☿ obser^{d}.	Difference of R.A. of Centres, ☿ − ⊙	Computed from N.A.	C − O.
h m s	h m s			s	s	s
11 1 42·9	20 8 42·0	3	I.	−42·338	−42·432	−0·094
11 9 42·7	20 16 41·2	3	II.	−45·188	−45·354	−0·166
11 15 21·7	20 22 19·0	3	II.	−47·442	−47·414	+0·028
11 34 1·2	20 40 55·5	2	I.	−54·044	−54·222	−0·178
11 37 31·1	20 44 24·7	3	I.	−55·267	−55·498	−0·231

The following differences of declination were also observed and are similarly corrected and compared with the *Nautical Almanac:*—

Durham Sid. T.	Green. M.T.	No. of Measures.	Limb observed. ☿ ⊙	Difference of Decl. of Centres.	Computed from N.A.	C − O.
h m s	h m s			′ ″	′ ″	″
11 25 41·2	20 32 36·7	3	N.L. − S.L.	−7 41·11	−7 37·46	+3·65
11 27 26·5	20 34 21·8	2	S.L. − S.L.	−7 35·73	−7 32·94	+2·79

An attempt was also made to measure the diameter of *Mercury.* The micrometer employed was a double image micrometer of Mr. Airy's original construction. The value of its screw was determined in March 1868, and found to be 18″·693, a value closely agreeing with that determined some years ago by the Rev. R. Thomson. The mean of eight measures gives for the diameter 9″·001, which is a somewhat small result. Owing to the rapidity with which the observations were necessarily taken, and the rather unsatisfactory image of the planet, this result cannot be considered of any great value; the individual measures are, however, very fairly accordant.

On the Transit of Mercury. By W. Lassell, Esq.

1868, Nov. 4, 20½ hours. Observed part of the transit of *Mercury*. Telescope used, a Gregorian of 4·7 inches aperture, power 120. The Sun not visible over the trees until about $19^h\ 50^m$, and then too much disturbed to see anything well. At $20^h\ 20^m$ undulation less, but still the planet and Sun's limb greatly agitated. At $20^h\ 55^m$ settled myself at the telescope to observe last contacts, though from the state of the limbs no *accurate* observation could be expected. The nearest times which it was possible to note were the following:—

	h	m	s
First contact	21	0	12
Planet apparently bisected at	21	1	12
Last contact, best observed of the three ..	21	2	22·8
Times by watch, which was slow for G.M.T.	0	0	25

Assumed places of Ray House, Latitude 51° 31′ 23″
Longitude $2^m\ 49^s{\cdot}3$ west.

Some fine spots were on the Sun, one towards the eastern limb, almost exactly the same size as the planet; but the contrast of shade was so striking as to give an optical proof how very far even the central part of a solar spot is from absolute blackness.

Ray Lodge, Maidenhead,
November 5th, 1868.

On the Transit of Mercury. By F. C. Penrose, Esq.

Thinking that, amongst the numerous and far more important communications which you will receive of the observations of the transit of *Mercury*, there may yet be some value for one made with a small aperture and moderate magnifying power — the value, if any, being to illustrate the question of the transit of *Venus* in 1769 — I beg to communicate the following observation:—

Focal length	30in.
Aperture	2¼

Concave single lens eye-piece; magnifying power 70.

I do not doubt that my G.M.T. would be true to half a second. The error could not exceed a second.

The state of the definition fairly good with the power employed, but would not have suited a higher. The Sun's limb boiled all along, but not very violently.

	G.M.T. h m s
Thread of light interrupted at internal contact	21 0 53·7
Boiling recommenced along the limb after the planet, and 10ˢ after the notch ceased to be visible	21 2 37·9

Both observations appeared very sharp and sudden, but the last observation was the best.

The time which elapsed between these observations was

$$1^m\ 44^s{\cdot}2$$

the calculated duration appears to be

$$2^m\ 34^s{\cdot}8$$

and the time of final egress for this station

$$21^h\ 2^m\ 57^s{\cdot}7.$$

It appears, therefore, that irradiation and other circumstances connected with the smallness of the telescope and low power employed, reduced my observed duration by $50^s{\cdot}6$; and this effect was more considerable at internal than at external contact. And this nearer coincidence in the latter case may, perhaps, be accounted for by the diminution of the disturbing effect of the atmosphere on the Sun's rays in the line of vision, by the intervention of the planet.

Wimbledon, Nov. 11, 1868.

On the Transit of Mercury. By James Buckingham, Esq.

On the morning of November 5th, 1868, excellent observations were made on *Mercury* during his transit over the Sun's disk, as from the time of sunrise till the end of the transit there was no trace of a cloud near, but a fine clear sky, and, about half-an-hour after the Sun had risen, the air became very steady. The contacts were both seen on the preceding limb of the Sun, and noted with great exactness by the chronograph at the following times, viz. :—

	h m s	
Internal contact at	11 59 8·61	Sidereal local time.
External contact at	12 1 59·09	„ „

Or in Greenwich mean time :—

	h m s
	21 0 18·77
	21 2 40·85
Duration	0 2 22·08

The internal contact occurred very suddenly by an apparent elongation of the planet's semi-diameter near the place of contact, when, by the previous appearance of the streak of light of the Sun, I had expected some ten to twelve seconds would have elapsed before the contact would occur; but at the time noted the circularity of *Mercury* was lost at point of contact (when all light of the sun at that part disappeared) as roughly shown in the sketch.

During the transit the image of the planet was beautifully defined and round, absolutely *black* without the slightest *penumbra*. During my observations, twice there existed an unsteadiness, and I noticed the image of *Mercury* expand and contract rapidly; but, during a long steady period, I obtained ten sets of measures of the planet's diameter, with a power of 340 on my 9-inch Equatoreal; the mean of them gave 7″·651, differing 2¼″ from that given in the *Nautical Almanac;* I used the micrometer (parallel wire) on my larger instrument, power 610; the result of five sets of measures differed only 0·3 from the previous (more or less, viz. 7″·618), and I am quite satisfied these measures are approximately true, but the great discrepancy for some moments very much surprised me. Yet, on reflection, I think it quite consistent that the observed diameter of a planet in transit should appear less than the true, owing to the effect of irradiation from the intense light of the Sun appearing to encroach on the dark body; and this supposition, I apprehend, is consistent, because so soon as the actual limb of the planet had really covered the true limb of the Sun all irradiation existing up to that moment (and thereby reducing the apparent diameter of the planet) would cease, and the *true semi-diameter of the planet* would be apparent and cause the appearance of elongation as described.

	°	′	″	
Observatory	51	29	13	N. Lat.
	0	4	58	W. Long.

I have thought it possible these few remarks may interest the Society, and beg to submit them.

Westmoreland House, Walworth Common.

On the Transit of Mercury. By C. L. Prince, Esq.

The weather this morning was favourable for observing the latter part of the transit of *Mercury* over the Sun's disk.

The definition was unusually bad for some time after sunrise, but at eight o'clock it much improved, and the planet appeared as a circular black spot on the Sun's disk. Its colour was much more intense than the nuclei of the solar spots which were present. I observed nothing worthy of note till the planet was

approaching internal contact, when a sudden elongation or lengthening out of the planet's horizontal diameter, almost to a point, occurred; and internal contact appeared to happen sooner than might have been expected from the previously circular form of the planet. This distortion of the figure of the planet continued till its centre was upon the Sun's limb; when it ceased. During the remainder of the egress, even to the moment of last contact, no distortion whatever occurred. The telescope I employed was my Equatoreal of 6·8 inches aperture and 12 feet focal length. The magnifying power was 210 in a Dawes solar eyepiece, with blue glass shade. The times of contact were as follows:—

	h	m	s	
The Internal Contact took place at	9	0	54·1	Local Mean Time.
The External ,, ,,	9	3	11·6	,,

My Observatory, in time, is 24″ East of Greenwich.

Uckfield Observatory, Nov. 9, 1868.

On the Transit of Mercury. By G. Williams, Esq.

Having been more fortunate than some of my neighbours in seeing the recent transit of *Mercury*, I venture to send a drawing and some measures I obtained of the planet's position on the Sun's disk.

A parallel wire micrometer was made use of, the Sun's limb being made to run along the upper moveable wire; the difference in time of transit between the Sun's preceding limb and the planet over the fixed wire was then taken; the time at once noted and the lower moveable wire brought up as quickly as possible to the planet's position.

The revolutions of the micrometer-screw were reduced to seconds in the usual way, and the difference in the times of transit converted into seconds of arc, and corrected by the cosine of the declination.

I once fancied I saw a ring of ruddy coloured light round the planet, but, for the greater part of the transit, nothing of the kind was visible.

There was no apparent elongation, pear shape, or black drop at the egress of the planet; but the boiling of the limb, which was considerable, may account for the absence of these appearances.

The telescope used was the equatoreal refractor, 4·25 inches aperture and 63 inches focus, by Messrs. Cooke and Sons; powers 71 to 140.

2 *Devonshire Road, Prince's Park, Liverpool,*
Nov. 11, 1868.

The figure which accompanies the foregoing Note shows the following three positions of the planet in regard to the Sun's disk, viz.—

	h m s	x = ″	y = ″
At	20 15 54	288·67	474·27
	20 26 56	259·81	496·90
	20 54 26	121·24	570·88

where x is the distance in the direction of a parallel of declination, and y the distance in the direction at right angles to this, from an origin outside the Sun's disk, such that in regard to it the co-ordinates of the Sun's centre are

$$x = y = \text{Sun's radius} = \tfrac{1}{2} \cdot 1941''{\cdot}4;$$

and further, that at egress the times of internal contact and of external contact were $21^h\ 0^m\ 18^s$ and $21^h\ 2^m\ 26^s$ respectively.—Ed.

On the Transit of Mercury. By Capt. Noble.

In common, I suppose, with every possessor of a telescope in England, I observed the transit of *Mercury* on the morning of Thursday, November the 5th.

Most unfortunately, through a circumstance over which I had no control, I had to withdraw my eye from the instrument at the instant of last internal contact; but, on re-applying it to the telescope immediately after this had taken place, I then noticed that the planet seemed to be somewhat drawn out, or *squared*, as roughly shown in my sketch; the double inversion of which arises from my observation having been made with the "Hodgson" reflector. I watched *Mercury* across the Sun's disk with a power of 74, but exchanged this for one of 154. after internal contact had taken place, and with this observed the egress. The last external contact or final emersion occurred at

$$12^h\ 2^m\ 30^s \text{ L.S.T.} = \text{Nov. } 4,\ 21^h\ 3^m\ 0^s{\cdot}99 \text{ L.M.T.}$$

a determination which I scarcely think is 1 second ± in error. I employed, as usual, my 4·2-inch Equatoreal of 61 inches focal length in making the observation.

Forest Lodge, Maresfield, Sussex,
12 Nov. 1868.

The transit of *Mercury* was observed at Rome by the Rev. Father Secchi, thus:—

1868, Nov. 5, Rome M.T.

	h	m	s
Ring broken at	21	50	5·91
Planet out at	21	52	27·46

Four Photographs of the Sun and *Mercury*, taken at Ely on November 5, communicated by the Rev. Prof. Selwyn, were exhibited at the Meeting. Prof. Selwyn writes:—

"At the taking of the third of the series, the planet was close to the edge, and was visible on the negative; but I cannot be quite sure of his appearance in the print. We ought to have given a larger aperture, or longer exposure, to insure a good representation of *Mercury* in the comparative darkness of the Sun's limb, but it was not thought of. The fourth is given, at a later hour, to remove all doubt as to the nature of the round spot representing *Mercury*."

On the Appearance of Mercury at its Transit, November 5, 1868. By William Huggins, F.R.S.

As buildings intercepted the view of the Sun from my observatory before $8^h\ 30^m$, I determined not to attempt any measures of *Mercury*, but to devote the short time at my disposal to a careful scrutiny of the appearance which the planet might present when seen upon the Sun.

The Sun's edge was a little tremulous from atmospheric agitation, but the solar surface was so well defined that the bright granules of which it is composed could be distinctly seen. The planet appeared as a well-defined, round, black spot. Whilst carefully examining the immediate neighbourhood of the spot for the possible detection of a satellite, I perceived that the planet was surrounded with an aureola of light, a little brighter than the solar disc. The breadth of the luminous annulus was about one-third of the planet's apparent diameter. The aureola did not fade off at the outer margin, but remained of about the same brightness throughout, with a defined boundary. The aureola was not sensibly coloured, and was only to be distinguished from the solar surface by a very small increase of brilliancy.

Almost at the same moment that I first perceived the surrounding annulus of light, I noticed a point of light nearly in the

centre of the planet. This spot of light had no sensible diameter with the powers employed, but appeared as a luminous point.

These phenomena, the aureola and the spot of light on the planet, were distinctly visible as long as the transit continued.

The whole aperture, 8 inches, of the telescope was in use, with a prismatic solar eye-piece, and powers of 120 diameters and 220 diameters. A sliding wedge of neutral tint glass allowed the apparent brightness of the Sun to be rapidly varied, but with all parts of the wedge brought before the eye, the phenomena described above continued to be visible.

It is of importance to state that the aureola was seen most plainly when the darkest part of the wedge was before the eye. This observation is scarcely what would have been expected on the supposition that the aureola is merely an optical or ocular phenomenon, for the conditions when the Sun was greatly darkened were favourable for the discrimination by the eye of the existence of a small difference of illumination; but they were, in the same degree, unfavourable for a mere optical effect produced by contrast. I am sorry that I omitted to notice if the definition of the granular structure of the solar surface was equally good, immediately round the planet where the aureola appeared.

I watched carefully the bright point upon the planet as *Mercury* was passing off the Sun, to ascertain if this luminous spot could be seen after the planet had become invisible. I kept it steadily in view until the part of the planetary disc, where the point of light was situated, reached the Sun's limb, I then ceased to see it. This observation must be regarded as negative merely, and not as proving the invisibility of the luminous point when the planet had passed off the Sun, for so small a point of light might be easily overlooked when the form of the planet was no longer visible to serve as a guide to the eye.

It may be well to state that at the time of making these observations, the appearances described by Schröter, Moll, and others, which I had not read for some years, were absent from my memory. I was not looking for these appearances, and it was some little time before I would believe in their reality.

If *Mercury* be surrounded by a transparent atmosphere the solar rays would be bent in on all sides and would cross in front of the planet, but would then proceed in directions far too much removed from the line joining the Sun, *Mercury*, and the Earth, to be received by the telescope. Such an atmosphere, in consequence of its power of turning aside the solar beams incident upon it, should appear darker than the solar surface.

Similar phenomena have been observed at some former transits. A sort of ring of faint light was seen by Plantade at the transit of 1736; also by Proserpin; also by Flaugergues in 1786, and in 1789, and 1799. He calls it "un anneau lumineux." Mechain, Messier, Fritsch and Seyffler observed a similar phenomenon. It is also described by Schröter and Harding during the transit of 1799. In 1832 Dr. Moll saw it as "a nebulous ring of

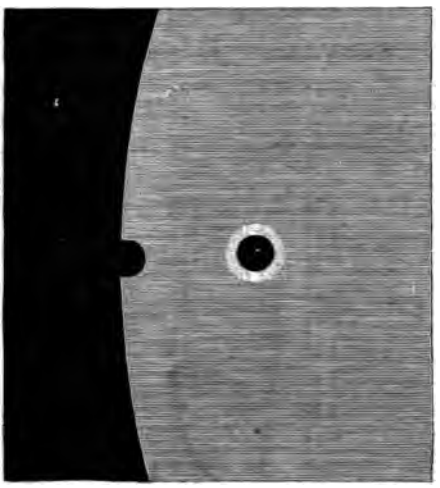
a darker tinge approaching to a violet colour." Some of these observers appear to have considered the aureola to be slightly brighter, and others as in a small degree darker than the Sun.

The other phenomenon, that of a bright spot on the planet, has been seen at former transits. At the same time that Schröter, in 1799, perceived the ring, he saw on the body of the planet a small bright spot, which was seen independently by Harding. At a later period of the day, Schröter saw the luminous speck, sometimes on one part of the disc and sometimes on another, being unable to keep it constantly in view.

A bright spot, not in the centre of the disc, was seen by Dr Moll at the transit of 1832.*

Professor Powell has proposed to account for the bright spot as a phenomenon depending on the diffraction of light.† As, however, in all the observations the spot was not seen in the centre of the planet's disc, this explanation seems to be scarcely applicable.

The following appearance was noticed almost immediately after the planet's disc came up to the Sun's limb. The spot appeared distorted, spreading out to fill up partly the bright cusps of the Sun's surface between the planet's disc and the sun's limb. This appearance increased as the planet went off the Sun, until when the disc of the planet had passed by about one-third of its diameter, it presented the form represented in the diagram, in which the



* See Dr. G. Moll on Transit of 1832, *Mem. R. Astron. Society*, vol. vi. p. 116; also, Breen, *The Planetary Worlds*, p. 138. I have been unable to refer to Schröter's original observations in the *Hermographische Fragmente*. This work is not in the library of the R. Astronomical Society, nor in that of the Royal Society.

† Prof. Powell on Irradiation: *Mem. R. Astron. Society*, vol. xviii. p. 69.

margin of the disc from points at the end of a diameter parallel to the Sun's limb, instead of continuing its proper curve, appeared to go in straight lines up to the limb, thus entirely obliterating the cusps of light, which would otherwise have been seen between the planet and the limb. In the diagram the aureola and the bright spot are not repeated in the figure of the planet on the Sun's limb.

A Determination of the Constant of Nutation from the Observations of Polaris in R.A. made at the Royal Observatory, Greenwich, from 1851–1867. By E. J. Stone, Esq.

In the June number of the *Monthly Notices* of the Royal Astronomical Society appears an abstract of a paper of mine on "A Determination of the Constant of Nutation from the Observations in N.P.D. of *Polaris*, *Cephei* 51, and *δ Ursæ Minoris*, made with the Transit Circle of the Royal Observatory, Greenwich, 1851–1865." In the present paper I have discussed the observations of *Polaris* in R.A. made with the Transit Circle of the Greenwich Observatory, from 1851 to 1867, both inclusive.

The observations, as extracted from the yearly Catalogues, are given in Table I.

Mean Right Ascensions of *Polaris* for Jan. 1, extracted from the Annual Catalogues.

Year.	Fraction of Year.	No. of Obs.	R.A. h m s
1851	0·46	99	1 5 19·02
1852	0·43	128	1 5 36·75
1853	0·61	76	1 5 54·65
1854	0·53	169	1 6 12·98
1855	0·60	110	1 6 30·86
1856	0·48	113	1 6 49·36
1857	0·52	150	1 7 7·37
1858	0·47	191	1 7 25·90
1859	0·55	155	1 7 44·23
1860	0·46	92	1 8 3·17
1861	0·50	116	1 8 21·50
1862	0·59	67	1 8 40·52
1863	0·48	78	1 8 59·63
1864	0·47	112	1 9 19·08
1865	0·52	110	1 9 38·06
1866	0·53	93	1 9 57·87
1867	0·64	77	1 10 17·34

The clock-errors from 1851 to 1855 were determined from the right ascensions of fundamental Stars, as given in the Greenwich

twelve-year Catalogue: those from 1856 to 1861 from the R.A.'s of the Greenwich six-year Catalogue: those from 1862 to 1867 from the R.A.'s of the Greenwich seven-year Catalogue. To bring the system to uniformity, the R.A.'s from 1851 to 1855 will be increased by $+0^s.013$: those from 1856 to 1861 by $+0.004$.

The Nutation constant adopted during the years 1851–1856 was $9''.25$. The constant from 1857 to 1867 was $9''.224$. The following corrections will be applied for the effect of this difference of adopted constants:—

		s
To the Results of the Year	1851	+0·018
	1852	−0·004
	1853	−0·029
	1854	−0·045
	1855	−0·060
	1856	−0·066

The following are the corrections applied to the results of each year to allow for the precessional motion in R.A. from the year 1859. The Pulkova constants have been used:—

		m	s
Correction to Result for	1851	= 2	24·397
	1852	= 2	6·766
	1853	= 1	49·018
	1854	= 1	31·151
	1855	= 1	13·165
	1856	= 0	55·058
	1857	= 0	36·829
	1858	= 0	18·477
	1859	= 0	0·000
	1860	= 0	18·604
	1861	= 0	37·336
	1862	= 0	56·195
	1863	= 1	15·184
	1864	= 1	34·306
	1865	= 1	53·561
	1866	= 2	12·951
	1867	= 2	32·477

Then, assuming

Proper Motion in R.A.	$= 0^s.1148 + p$
True Constant of Nutation	$= 9''.224 + y$
Mean R.A. of Polaris, 1859, Jan. 1	$= 1^h\,7^m\,44^s + x$

we have the following equations of condition for the determination of x and y:—

					w.
1851	x	$+ 0{\cdot}680\, y$	$=$	$0{\cdot}366 + 8\,p$	99
1852	x	$- 0{\cdot}141\, y$	$=$	$0{\cdot}329 + 7\,p$	128
1853	x	$- 1{\cdot}119\, y$	$=$	$0{\cdot}341 + 6\,p$	76
1854	x	$- 1{\cdot}720\, y$	$=$	$0{\cdot}673 + 5\,p$	169
1855	x	$- 2{\cdot}315\, y$	$=$	$0{\cdot}437 + 4\,p$	110
1856	x	$- 2{\cdot}543\, y$	$=$	$0{\cdot}700 + 3\,p$	113
1857	x	$- 2{\cdot}523\, y$	$=$	$0{\cdot}433 + 2\,p$	150
1858	x	$- 2{\cdot}232\, y$	$=$	$0{\cdot}496 + p$	191
1859	x	$- 1{\cdot}623\, y$	$=$	$0{\cdot}234$	155
1860	x	$- 0{\cdot}935\, y$	$=$	$0{\cdot}455 - p$	92
1861	x	$- 0{\cdot}035\, y$	$=-$	$0{\cdot}062 - 2\,p$	116
1862	x	$+ 0{\cdot}920\, y$	$=-$	$0{\cdot}019 - 3\,p$	67
1863	x	$+ 1\,608\, y$	$=-$	$0{\cdot}013 - 4\,p$	78
1864	x	$+ 2{\cdot}212\, p$	$=+$	$0{\cdot}200 - 5\,p$	112
1865	x	$+ 2{\cdot}587\, y$	$=-$	$0{\cdot}190 - 6\,p$	110
1866	x	$+ 2{\cdot}652\, y$	$=+$	$0{\cdot}115 - 7\,p$	93
1867	x	$+ 2{\cdot}369\, y$	$=-$	$0{\cdot}055 - 8\,p$	77

Multiplying each equation by the corresponding value of ν and reducing the equations by the method of least squares, have for the determination of x and y the equations —

$$1936\ x - 866{\cdot}518\, y = 581{\cdot}466 + 935\ p$$
$$-866{\cdot}518\, x + 6986{\cdot}703\, y = -980{\cdot}016 - 11340{\cdot}24\, p$$

From which

$$x = + 0{\cdot}252 - 0{\cdot}26\,.\,p$$
$$y = - 0{\cdot}109 - 1{\cdot}66\,.\,p$$

or Mean R.A. of *Polaris*, 1859, Jan. 1,

$$= 1^{h}\ 7^{m}\ 44^{s}{\cdot}252 - 0{\cdot}26\, p$$

$$\text{Constant of Nutation} = 9''{\cdot}115 - 1{\cdot}66\, p$$

If we adopt for all the Proper Motions the values given "Gould's Standard Places of Fundamental Stars, *second editio* we have for

Proper Motion in R.A. of Polaris	$=$	$0^{s}{\cdot}1148 + p$
N.P.D. of Polaris	$=$	$-0{\cdot}004 + \delta p$
N.P.D. of Cephei 51	$=$	$+0{\cdot}048 + \delta c$
N.P.D. of δ Ursæ Minoris	$=$	$-0{\cdot}043 + \delta m$

The following will then be the resulting values of the const. of Nutation: —

(1) $N = 9''.115 - 1.66\ p$ from the R.A. of Polaris
(2) $N = 9.109 + 6.1\ \delta p$,, N.P.D. of Polaris
(3) $N = 9.143 - 3.6\ \delta c$,, N.P.D. of Cephei 51
(4) $N = 9.214 + 1.75\ \delta m$,, N.P.D. of δ Ursa Minoris.

Giving (1) and (2) twice the weights of (3) and (4), we have for the final value of the constant of Nutation,

$$9''.134 - 0.55\ p + 2.02\ \delta p + 0.29\ \delta m - 0.6\ \delta c.$$

Occultation of Stars by the Moon, 8 *Sept.* 1868.
By J. Joynson, Esq.

		h m s	
71 Tauri	Dis.	11 31 17·7	G.M.T.
θ^1 Tauri	Dis.	12 40 16·5	
	Re.	13 41 25·5	
θ^2 Tauri	Dis.	12 46 15·4	
	Re.	13 36 7·1	
81 Tauri	Dis.	13 45 6·8	
	Re.	14 18 8·6	
1391 B.A.C.	Dis.	14 10 21·8	
85 Tauri	Re.	14 57 20·7	

The occultation of α *Tauri* could not be observed owing to dense cloud.

Waterloo, near Liverpool.

Mr. Chambers has communicated the following Errata in his "Catalogue of Binary Stars," *Monthly Notices*, vol. xxviii. pp. 174–179.

Part I.

The columns of Epoch should be headed 1800 +, *not* 1880 +
No. 4. 34 *Andromedæ* should be 36, and the Mag. of A, 6, *not* 4.
No. 31. The Epoch should be '63·1, *not* '66·1.
No. 80. Struve's synonym should be 3127, *not* 2137.

Part II.

No. 19. The Epoch should be '64·7, *not* '65·7.
No. 31 is misplaced in R.A.; it should follow 28.
No. 42. The Decl. should be 21° 36′·3, *not* 21° 26′·3.
No. 43. The Magnitude of B should be $7\frac{3}{4}$, *not* $8\frac{1}{4}$.

Bickley, Kent, August 6th, 1868.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *December* 11, 1868. No. 2.

Admiral MANNERS, President, in the Chair.

Rev. C. Gribble, British Embassy, Constantinople;
Dr. G. Neumeyer, 10 Gower Street; and
David Vines, Esq., Kingswood, Bristol,

were balloted for and duly elected Fellows of the Society.

On the Preparatory Arrangements which will be necessary for efficient Observation of the Transits of Venus in the years 1874 and 1882. By George Biddell Airy, Astronomer Royal.

On two occasions (*Monthly Notices*, 1857, May 8, and 1864, June 10) I have called the attention of the Society to the Transits of *Venus* across the Sun's disk, which will occur in the years 1874 and 1882; and have pointed out that, for determination of the difference between the Sun's parallax and the parallax of *Venus*, the method by observation of the interval in time between Ingress and Egress at each of two stations at least, on nearly opposite parts of the Earth (on which method, exclusively, reliance was placed in the treatment of the observations of the Transit of *Venus* in 1769), fails totally for the Transit of 1874, and is embarrassed in 1882 with the difficulty of finding a proper station on the almost unknown Southern Continent.

The publication of M. Le Verrier's new Tables of *Venus*, and

of Mr. Hind's inferences from them as to the points of the Sun's limb at which the Ingress and Egress will take place in each Transit (which inferences I have in part verified), has induced me again to examine the whole subject. And, without giving up the hope of using the observation of interval between Ingress and Egress at each of two stations in 1882, I have come to the conclusion (from all the information which has reached me) that it will be unsafe to trust exclusively to the chance of securing observations on the Southern Continent; and that, while observations are by all means to be attempted in that manner, it is also very desirable to combine with them observations of the same phenomenon (at one time the Ingress, at another time the Egress), made at nearly opposite stations whose longitudes are *accurately* known, and recorded in *accurate* local time. This principle being once admitted, the Transit of 1874 is, or may be, as good for observations of that class as the Transit of 1882; and the selection of localities for the observations must be made with equal care for the two Transits.

Mr. Hind's calculations for the points of Ingress and Egress are contained in a paper in the *Comptes Rendus* of the French Academy for 1861, July 22. My valued friend has been so much accustomed, in the admirable calculations issued from his office, to describe phenomena as seen in an inverting telescope (a method of exhibition which I could wish entirely to banish) that he has inadvertently given the elements of Ingress and Egress on that supposition, but without explanation of it. Correcting for this peculiarity, the elements to be used for discussion of observation-stations are, with sufficient approximation, the following:—

Transit of Venus, 1874, *Dec.* 8. *Sun's Declination* 22° 35′ *S.*
Ingress at $14^h\ 0^m$ Greenwich Mean Solar Time.
Position on Sun's Limb 131° from South through East towards North.
Egress at $18^h\ 12^m$ Greenwich Mean Solar Time.
Position on Sun's Limb 160° from South through West towards North.

Transit of Venus, 1882, *Dec.* 6. *Sun's Declination* 22° 44′ *S.*
Ingress at $2^h\ 5^m$ Greenwich Mean Solar Time.
Position on Sun's Limb 35° from South towards East.
Egress at $8^h\ 2^m$ Greenwich Mean Solar Time.
Position on Sun's Limb 66° from South towards West.

With these data, a common terrestrial globe can be adjusted for each place, on the supposition that the Sun is vertically above the globe, and therefore that the globe-horizon defines those parts of the earth to which the phase will be visible. If a point be taken on the globe-horizon corresponding to the "Position on Sun's Limb," the terrestrial station thus indicated is that at which Parallax has its maximum effect in apparently displacing

Venus towards the Sun's centre; and the opposite station is that where the maximum apparent displacement from the Sun's centre occurs. The effect at any other station is proportional to the cosine of its distance, in arc of great circle of the Earth, from the maximum station.

The Maps which I attach to this paper have been prepared by taking advantage of the principle which I have described. They do not profess any extreme accuracy, but they will, I trust, be found abundantly accurate for the purpose for which they are prepared. All are on the same scale except the first, whose scale is $\frac{3}{4}$ that of the others. It will be remarked that the local elements, which at any station are important, are, first, the "Factor of Parallax," or the cosine of angle above mentioned, on which depends the theoretical value of the observation; second, the "Sun's elevation above the horizon," on which the chance of good observation will greatly depend. These are defined upon the maps; for the first, by the station's place among the semicircular curves; and for the second, by the station's place among the parallel lines. A point surrounded by a circle, upon the horizon-line, indicates the place at which the effect of Parallax is maximum.

It will be remarked that, in this mode of observing the Transit, the Ingress is a phenomenon complete in itself, requiring to be observed at nearly opposite sides of the Earth; namely, at one side where it is apparently accelerated by Parallax, and at another side where it is apparently retarded by Parallax. The two maps corresponding to these two tracts of observation will be placed in the bound volume, face to face. In like manner, the Egress is a phenomenon complete in itself, also requiring two districts of observation on nearly opposite sides of the Earth, different from those used for the observation of Ingress, and therefore requiring two special maps, and these in like manner will be bound face to face.

I can now proceed to discuss the individual maps with reference to the selection of stations for the observation of the several phenomena.

Transit of Venus, 1874, Dec. 8.

Map No. 1.—*Stations for observing the Ingress as accelerated by Parallax.*

Owhyhee and the neighbouring islands are excellent. The factor of parallax is about 0·92, and the Sun is at nearly two hours' elevation. There is English society at Woahoo. These islands are just within the tropics. For use of this station the absolute longitude must be accurately determined.

At the Marquesas Islands the Factor of Parallax is 0·7, and the

Sun is nearly as high as at Woahoo. Our neighbours across the Channel have, from the time of Louis XIV., taken an honourable lead in scientific enterprise of every class. I trust that we may rely on them for accurate determination of longitude at Marquesas, and for accurate observation of the Ingress in 1874.

The desert Aleutian Islands can scarcely be recommended, although the factor 0·8 for the westernmost of them, where the Sun is highest, is favourable. But it is very probable that the Russians will soon have established telegraphic communication with the mouth of the Amoor, by which its absolute longitude will be accurately determined; and though the factor is only 0·57, the Sun is 15° high, and the station will be valuable.

On the whole, if the British Government will undertake the accurate determination of longitude of Woahoo, and the careful observation of Ingress there in 1874, we may consider that good provision is made for the Accelerated Ingress.

Map No. 2.—*Stations for observing the Ingress as retarded by Parallax.*

The best station, as referred to the test of numbers, is Kerguelen's Island, where the factor is 0·91, and the Sun is 25° high. This island is emphatically known as "The Island of Desolation." I know not whether its character is so repulsive, or its utility as a zero of longitude so small, as to make our nautical authorities unwilling to determine its longitude, and to station observers there in 1874. If these difficulties are not thought too great, it will be an excellent position. At Crozet's Islands the factor 0·98 is very favourable, but the Sun is rather low (10° altitude).

The next stations in order of merit are Rodriguez, Mauritius, and Bourbon. Mauritius possesses this claim, that it will be a fairly good station, though not so good as Bourbon, in 1882; in 1874, as well as in 1882, it has this disadvantage, that the Sun will be low. If only one longitude can be determined in this chain of islands, it ought to be that of Mauritius; if two can be determined, they ought to be those of Rodriguez (for 1874) and Bourbon (for 1882).

At Madras and Bombay the factors, 0·47 and 0·44, are small; but the value of either station does not depend entirely on its simple factor, but upon the sum of its factor with the factors at the stations in Map No. 1. These two Observatories, with well-known longitudes, will prove very useful stations.

With the assistance which we may hope to receive from the British Government, we may consider the observation of the Retarded Ingress as well secured.

Map. No. 3.—*Stations for observing the Egress as accelerated by Parallax.*

Excluding from consideration the Southern Continent, as not to be entertained in our thought without the most absolute

necessity, the stations in order of merit are the Auckland Islands, Canterbury, Wellington, and Auckland, in New Zealand (factors ranging from 0·83 to 0·77), Norfolk Island (0·66), Melbourne and Sydney (0·6). I omit Chatham Island, where the Sun is rather low. The existence of the Observatories at Melbourne and Sydney makes the observation of the Accelerated Egress almost secure, although, in confirmation, I should much desire to have one station at least on the New Zealand group.

Map No. 4.—*Stations for observing the Egress as retarded by Parallax.*

The stations which are favourable for this observation are almost entirely on Russian and Turkish territories. At none of them is the factor less than 0·84; and we have, therefore, only to consider the elevation of the Sun, leaving to the national governments to estimate the facilities or difficulties depending on the locality, the climate, or the season. Any station either to the east or to the west of the Lower Caspian will have the Sun well elevated. Omsk, Orsk (whose longitude has been determined with peculiar care), Astrakhan, Erzeroum, Aleppo, Smyrna, and Alexandria, have the Sun sufficiently high. At Tobolsk, Perm, Kazan, Kharkov, Odessa, Constantinople, and Athens, the Sun will be rather low, and at Moscow it will be on the horizon. We may with the utmost confidence leave the selection of the stations, the determination of longitude, and the observation of the phenomenon, to our Russian friends. One station, however, ought specially to be considered as being, for this purpose, in British hands, namely, Alexandria. It appears not improbable that we may soon have very direct telegraphic communication with Alexandria; but failing this, I trust that no efforts will be wanting to determine accurately its longitude; a longitude which was in the survey of Admiral Smyth, and which always must be, the zero of longitude in the Levant. This being ascertained, Alexandria would probably be the best of all the stations for observation of the Retarded Egress.

Transit of Venus, 1882, Dec. 6.

First, by the method of absolute Longitudes.

Map No. 5.—*Stations for observing the Ingress as accelerated by Parallax.*

Omitting for the present all allusion to the Southern Continent, it will be seen that the best station is Kerguelen's Island, its factor being 0·98, and the Sun's elevation (12°) probably sufficient. This circumstance, in addition to its value as explained in the discussion of Map No. 2, renders it well worthy of attention. At Crozet's Islands the factor is 0·9, and the Sun's elevation 23°; abstractedly it is preferable to Kerguelen's Island,

but not in quite so great a degree as that in which Kerguelen's Island is superior in Map No. 2. The next in value are Bourbon and Mauritius, with factor about 0·78, the Sun being higher at Bourbon. On comparing these qualifications with those remarked under Map No. 2, the reasons will be evident for my recommendation that either the longitude of Mauritius or the longitudes of Bourbon and Rodriguez should be determined.

At the Cape of Good Hope the factor is about 0·62, and the observation there will be valuable.

The satisfactory observation of the Accelerated Ingress requires, however, some longitude-determinations.

Map No. 6.—*Stations for observing the Ingress as retarded by Parallax.*

Every city near the seaboard of the United States of America, and every important city of Canada, commands this phenomenon most favourably. The lowest factor is 0·95, and the smallest elevation of the Sun is 12°. The utmost reliance may be placed on the zeal of our American brethren for observing the Ingress. As great facility exists for determining the absolute longitude of any place within the range of American telegraphs (Harvard having been accurately referred to Greenwich), it is unnecessary to look further. Otherwise it might be remarked that Bermuda, Jamaica, and the West Indian Islands, and both sides of Central America, are excellent stations, but requiring determinations of longitude.

Map No. 7.—*Stations for observing the Egress as accelerated by Parallax.*

All the American stations mentioned in the last paragraph, from Halifax to New Orleans, and Bermuda and the West Indian Islands, are well situated for this observation, the factors being near 0·85, and the Sun's altitude varying from 4° at Halifax to 32° at New Orleans and Jamaica. The coast of South America also is favourable, from its union with the Isthmus to the harbour of Rio de Janeiro. It is believed that efforts have been made for exact determination, in a nautical sense, of the longitude of Rio; it may now be desirable to give to that longitude the utmost accuracy.

Map No. 8.—*Stations for observing the Egress as retarded by Parallax.*

Omitting for the present the Southern Continent, this observation will be amply secured by the Observatories of Sydney and Melbourne, where the factor is 0·96, and the Sun's elevation 12° to 14°. If, however, the longitudes of the New Zealand stations can be ascertained, they, with factor 0·8 and Sun's elevation 32°, will form a valuable addition.

Second, by the Method of interval between Ingress and Egress.

On comparing Maps Nos. 6 and 7, it will be seen that the North American localities supply, in a manner which leaves nothing to be desired, the demand for stations, at which the Ingress is retarded and the Egress accelerated, or the whole interval is diminished, by Parallax.

With these, it is necessary to combine one or more stations, at which the Ingress is accelerated and the Egress retarded, or the whole Interval is increased, by Parallax. On examining Maps Nos. 5 and 8, it will be seen that the only possible method of responding to this demand is by the selection of stations on the Antarctic Continent, in which the observation will be made when the Sun is nearly below the Pole.

In so far as the coast of the Antarctic Continent follows nearly a parallel of latitude, the best position for a station is at 7^h east longitude. The factors would be, for Ingress and Egress, about 0·95 and 0·68. The Sun would be, at each station, about three hours from the sub-polar meridian. But its elevation above the horizon would scarcely exceed 4°, and any alteration of the longitude, with the view of increasing the elevation at one phenomenon, would diminish it at the other.

Advantage may, however, be taken of the deep southern inlet discovered by Sir James Ross, to the western side of which is given the name of South Victoria. If a station can be established in latitude exceeding 72° S., it will be preferable, for observation of Ingress, to the station in 7^h longitude, and if the expedition could be pushed on to an observing place in the neighbourhood of Mounts Erebus and Terror, that position would be greatly preferable. For observation of Egress, it is manifestly far superior; the Sun's altitude being about 27°. The factors for the two observations are respectively about 0·78 and 0·58.

The decision on the choice to be made between these two stations, and the judgment on the facility, or even the possibility, of using either of them, must rest with persons who have had some familiarity with Polar, and if possible with South Polar voyages.

In partial correction of some small inaccuracies in these remarks, it may be observed that,—

The Ingress, as viewed from the Earth's centre, is always a few minutes earlier, and the Egress always a few minutes later, than is supposed in the maps.

As affected by Parallax, the phenomenon is always retarded with ascending Sun and accelerated with descending Sun.

As referred to Apparent Solar Time, the phenomena are slightly retarded.

The only phenomena which are critically affected by these corrections are those of Maps Nos. 4 and 5, and in both the circumstances of solar elevation are rendered more favourable.

There may be a cause of uncertainty, in the observation-elements on which M. Le Verrier's Tables of *Venus* are founded, arising from the unsatisfactory form in which the observations of the planet were recorded in Bradley's time. At a critical Inferior Conjunction, when *Venus* was apparently a very large body, very deeply hollowed, it is impossible to say whether the limb was observed or whether an attempt was made to observe the centre. In my reductions of the planetary observations I inclined to the former idea. Subsequently M. Le Verrier adopted the latter. The tables are, at this time, very accurate; and it may prove that M. Le Verrier's interpretation was correct. Any uncertainty, however, of this kind, makes it desirable to avoid observations of the Ingress or Egress very near to the horizon.

It appears, from this discussion, that the Absolute Longitudes required for the observation of the two Transits, the determination of which may be considered as a duty of the British nation, are the following:—

Alexandria.
Stations in New Zealand.
Woahoo, or some other point in the Sandwich Islands.
Kerguelen's Island, or Crozet's Islands (Kerguelen's being slightly preferable.)
Mauritius, or the two islands Rodriguez and Bourbon (which would be preferable).

I now proceed to remark on the methods by which these longitudes may be determined.

For Alexandria,—we may probably rely on the transmission of Greenwich Time, either by sending numerous chronometers in the steam ships which make regular voyages between the British ports and Alexandria, or by galvanic signals through the desired direct telegraph-communication. The saving of time and labour by the latter method would be so great that, if there is any reasonable prospect of soon forming that communication, it would be best to wait for it. The Local Time may be obtained by instruments sent for the purpose, either a Transit Instrument for meridian transits, or an Altazimuth for extra meridian zenith distances. Where the labour of computation, as for a limited object, is not worth taking into account, I prefer the Altazimuth. The largest portable instrument, with circles about sixteen inches in diameter and with four microscopes for reading the vertical circle (two microscopes being insufficient for eliminating the effect of oval form) should be employed; an instrument of this class now form

part of the equipment of the Royal Observatory of Greenwich. In every expedition, however, I would send instruments of both classes. It is unnecessary to particularise clocks and chronometers, &c.

For the New Zealand stations, I should trust that connexion by chronometers might be made with Sydney and Melbourne.

For Woahoo and Kerguelen's Island, the circumstances are different. The expense of sending special steamers with chronometers, even from San Francisco to Woahoo, and from the Cape of Good Hope to Kerguelen's Island, would be very great. Here, I think, we must make use of the motions of the Moon. If a hundred transits of the Moon be taken, equally divided between the first and second limb, the probable error of the resulting longitude will be about three times the probable error of a single transit of a star ; or, with liberal allowance for every source of error, it will be below 1^s of time : this accuracy would suffice. A hundred transits might be obtained in a year with the transit instrument, or perhaps in three months by vertical transits with the altazimuth. Through this time, a competent observer must be stationed at the one point. No transmission of chronometers would be required.

The same method, I think, must be adopted for Mauritius, or for Rodriguez and Bourbon.

It would be a question for the Naval Authorities whether the determinations of Absolute Longitude should be preliminary to the observation of the Transits of *Venus*, or whether they should be made about the same time, requiring only one visit of the observer or observers to the station.

For British observation of the Transit of *Venus* in 1874, the number of sets of instruments required (each set including a transit instrument, an altazimuth, a clock, several chronometers, several telescopes, one or two observing tents) would be five (Woahoo, Kerguelen's Island, Rodriguez or Mauritius, Auckland, Alexandria.) For the Transit in 1882, the number required would be three (Kerguelen's Island, Bourbon or Mauritius, Auckland). I suppose here that the Canadian observatories are already sufficiently equipped. The instruments of the 1874 Expedition, thus set free from two stations, would be required at that of which I next speak.

For selection of an observing station on the Southern Continent, I think that it will be best to examine carefully the accounts given by Sir James Ross, and Commodore Wilkes, and their officers, and to decide on the absolute and relative probability of gaining access to the coast on December 6, in the neighbourhood of the Antarctic Circle at 7^h east longitude, or at a much higher latitude on South Victoria ; and also to judge on the possible difference in the transparency of the atmosphere. The choice of station being made, I would not recommend any reconnaissance, but I would propose that an expedition should

go direct to the selected point, in good time for the observation of the phenomenon. The season is early for south polar expeditions, and any difficulties produced by ice would probably diminish every day. A station being gained, all that is necessary in the way of subsidiary observation is, a few days' observation to give clock rate; then the clock times only of the two phenomena will furnish all that is required.

The first action to be undertaken by the Government is, to procure the stock of instruments; and this ought to be done without delay. An observing plant like that above mentioned is not to be obtained in haste; and the proposed expedition might be entirely crippled by a small negligence on this point. The equipment of ships, and the selection of officers, would probably require much less time.

I have now to advert to the details of the instruments and the observations of the Ingress and Egress.

I trust that my friend Mr. Stone (First Assistant of the Royal Observatory), whose knowledge of the accounts of the Transits of *Venus* in the last century probably exceeds that of any other astronomer, and whose personal experience in the observation of the late Transit of *Mercury* is equal to that of any other, will be induced to attach to this notice his own remarks on the points to which the observer's attention ought specially to be called. We are undoubtingly of opinion that the telescopes ought to be nearly similar, with aperture perhaps of 4 to 5 inches, and used with power perhaps of 120 to 200. When there are several telescopes at one station, some variation among them may be actually advantageous. The protection of the eye from the Sun's bright light, and the neutralization of the atmospheric dispersion, require distinct consideration. From my own experience in the observation of total eclipses, and from my view of *Mercury* in the late transit, I can state positively that much is gained in clearness of outline by the use of an achromatised prismatic coloured glass, adjustible by the observer. The atmospheric dispersion enters very seriously into the question of accurate observation. The very nature of a phenomenon, which is to exhibit in the highest degree the effects of parallax, implies, that the planet must immerge or emerge near the highest or lowest part of the Sun's limb, and that the Sun must not be very high. No circumstances can be imagined in which atmospheric dispersion is more injurious. To meet this, coloured shades should be used which, as examined on a solar spectrum, absorb the ends of the spectrum, leaving the green predominant. I think it probable also that advantage would be gained by attaching to the eye-piece a dispersive prism with its acute angle upwards; on this, however, I propose to make some experiments.

I trust that Mr. De La Rue will be induced to publish, with

this paper, his proposal in detail, for the use of photography in the register of a transit of *Venus*.

In terminating this communication, I may perhaps remark that the expedition sent into the Pacific for the observation of the Transit of *Venus* in 1769, has always been esteemed as one of the highest scientific glories of Britain in the last century; and I may be permitted to express my hope that it will be surpassed by the efforts which our country will make, with the same object, in the present century. And I close my paper with an extract from the appeal made by my illustrious predecessor, Dr. Halley, forty-five years before the earlier of the Transits then in view. The personal reflection, though applicable with less force to myself, cannot entirely escape my thought: —

"Ac sane vellem diversis in locis ejusdem Phænomeni observationes à pluribus institui, tum ad majorem adstruendam ex consensu fidem, tum ne Nubium interventu frustraretur singularis Spectator, eo spectaculo quod nescio an denuo visuri sunt hujus et subsequentis sæculi Mortales; et à quo pendet Problematis nobilissimi et aliunde inaccessi solutio certa et adæquata. Curiosis igitur syderum scrutatoribus quibus, nobis vitâ functis, hæc observanda reservantur, iterum iterumque commendamus, ut, moniti hujus nostri memores, observationi peragendæ strenue totisque viribus incumbant; iisque fausta omnia exoptamus et vovemus, præprimis ne nubili cæli importunâ obscuritate exoptatissimo spectaculo priventur; utque tandem Orbium cælestium magnitudines intra arctiores limites coercitæ in eorum gloriam famamque sempiternam cedant."

Royal Observatory, Greenwich,
December 3, 1868.

Remarks by Capt. Richards, Hydrographer to the Admiralty.

I can have no hesitation in responding to the President's invitation to offer such observations as may occur to me on the important subject which has occupied the attention of the Society this evening, viz. the approaching Transits of *Venus* over the Sun's disk on December 8, 1874, and December 6, 1882, undoubtedly, in an astronomical point of view, the most important events of the present century.

The Astronomer Royal has, in his accustomed able, lucid, and instructive manner, placed before us the objects to be attained and the means necessary to secure them; it would be quite unnecessary and out of place on my part to take up your time in supplementing these remarks of his.

We can all quite understand and sympathise with the anxiety which the Astronomer Royal must feel as to the precise action which our Government, when the time comes, will take on a question

of such vital importance to the astronomers of all nations; in ordinary affairs we do not think it necessary to look a-head so far as five years, but, as regards astronomical phenomena, concerning which there can be no uncertainty, and which do not recur but twice in a century, nothing must be left to chance which can possibly be provided for.

Mr. Airy and myself have been in communication on the subject for some months, and he has placed before the Admiralty, in the clearest manner, the arrangements which it will be necessary to make. Although I have no warrant for saying what precise steps will be taken;—indeed it would be impossible for the Admiralty to decide themselves at the present time,—yet I am justified in saying that such arrangements have been made as will insure that the whole question will be brought under their consideration at a sufficiently early period to enable the proper steps to be fully carried out; and it is impossible for any one who has followed the enlightened liberality which the Government of this country has ever adopted in such matters, to fear that Great Britain, which sent the expedition to the Pacific in 1768 for a similar purpose, will be behindhand on this occasion, when the increase in astronomical knowledge and the improvement in astronomical instruments offer such a guarantee of increased success. In regard to the absolute determination of the longitudes of the stations named by the Astronomer Royal as necessary for the observations of 1874, I cannot conceive that any difficulties or obstacles will arise, and the only immediate measures which appear necessary to be taken are in the preparation of the instruments, which undoubtedly should not be long delayed.

In regard to the selection of stations on the Antarctic Continent for the observations of 1882, this is a more serious question, and one involving considerable difficulties, though not, I apprehend, of such magnitude as is conceived by many. It would, however, certainly be necessary to make a reconnoissance in the summer of 1882, in the first place, because we know of no accessible station for a ship either in the longitude of 7 hours east, or in Victoria Land, which was noted by Sir James Ross, both of which places are mentioned by the Astronomer Royal as eligible, and in the second place, because the navigation in these regions is not open until some weeks after the time in which the transit will occur; therefore it would be necessary for the astronomers to pass a winter there, or from January, 1882, until January, 1883; this would be done either on board ship, or by landing the party, perhaps, on Possession Island, Victoria Land, on which Sir James Ross landed in 1841, or on one of the small islands to the south of it. My own opinion, looking to the uncertainty of finding a wintering station for a ship, is that the latter course would be the most feasible, and there would be little doubt of the facility of reaching one or other of these islands with a suitable steam-vessel, making Tasmania or New Zealand the base of operations; doubtless a year passed in this region would be most profitably em-

ployed in adding to our knowledge of magnetism, and various other branches of physical science.

Remarks by Capt. Toynbee.

The neighbourhood of Prince Edward's Islands, Crozet's, and Kerguelen's Islands, is, so far as my experience goes, subject to a great deal of thick weather, so that, although December may be a favourable season of the year, it would be well to have an observing station on each, in case of one or two being cloudy.

Remarks by Rear-Admiral Ommanney.

Speaking from experiences in Arctic navigation, I consider that to ensure obtaining a good position for the station of the ship on the Antarctic Continent from whence the observations are to be conducted, it is most desirable that a preliminary survey of the coast on the Antarctic Continent should be carried out before the year 1874, confining the survey more particularly to Victoria Sound, the locality for observing the transit.

Sir James Ross, in his memorable voyage, could do no more then than discover the coast line, the ships at his command were such that he could not hazard their safety by making any attempt to delineate the features of the coast, so that our knowledge of the Antarctic Continent remains where it was, of a most limited nature.

It is most important that those who will be engaged on this Expedition shall find, by previous exploration, the position for the ship to winter at, as well as to initiate the Commander and crew with some experience in the navigation of the Antarctic icy seas; without feeling our way, as it were, it would be hazarding this noble work to the risk of failure, were the expedition to proceed direct for making the transit observations with no more knowledge of the Antarctic regions than what we are now confined to.

We have not yet availed ourselves of steam-power for exploring these regions. I am of opinion that with a vessel properly constructed for the service, aided with steam-power, we could with confidence make a thorough search of the coast in Victoria Sound and the adjacent countries during the navigation season, making Van Dieman's Land the head-quarters of the Expedition.

An Expedition of this nature would be attended with many advantages for the interests of science, but most especially in training the officers for the service of the Transit Expedition when the time arrives, and knowing the nature of the position where the ship will have to take up her station, I know from experience how desirable it is that officers should have a special training for icy navigation.

I fully concur in all that has fallen from the Hydrographer of the Navy, and hope ere long to hear that operations are taking for sending out to explore the Antarctic Seas.

Remarks by Staff-Commander J. E. Davis.

Staff-Commander J. E. Davis had accompanied Sir James Ross on his southern voyage, and had himself landed at one of the suggested positions of the Astronomer Royal, and he believed there would be no difficulty whatever in again effecting a landing at the same place, viz., on Possession Island, off the coast of South Victoria, in latitude 72°; he did not agree with Admiral Ommanney that a reconnoissance and further survey of that coast was desirable, as no sign of a harbour whatever could be discovered by Captain Ross along the coast, the whole of the land being faced with ice as perpendicular as the wall of a room; indeed, so anxious was Captain Ross to find a place in which he could have wintered in the ships, that he declared he "would willingly give his right arm to find one."

With respect to the suggested station in 7^h east longitude he would observe that, although we were not certain of the position and delineation of the land in the vicinity, yet as all the land that had been seen had been described as mountainous, it was natural to suppose that such was its general character, and if so, the mountains would be immediately between the observer and the Sun; and when it was considered that the altitude of the last-named body at the times of Ingress and Egress, on either side of the pole, would only be between 4 and 5 degrees, the chances of seeing the Sun at all would be extremely problematical, and with the station on the coast of South Victoria in latitude 72° in view, at which no such difficulty existed, he thought it would be desirable to give up the position in 7^h east longitude as untenable.

With regard to the period of the season at which the Transit took place it was to be remembered that the 6th of December was so early that no ships had ever reached the Antarctic Circle by that date, and as it would be necessary to arrange the instruments, &c., preparatory to the observation, he might say that the ships ought to be on the spot at least a month before, this would be the 6th of November, a date altogether out of the question; and as the ships could not winter in the South, the party would necessarily have to be landed the year before: but with good huts he had no doubt they could pass the winter very comfortably; they would have a pleasant prospect before them, and plenty of penguins to live on.

In comparison with Kerguelen Island and the Crozet's, Capt. Davis considered the chances of observing the transit—meteorologically speaking—to be greatly in favour of South Victoria.

Capt. Davis stated that he was preparing a paper on the subject of Antarctic discovery and its connexion with the Transit of

Venus in 1882, for the Royal Geographical Society, in which he should enter more fully into the subject than he could then do.

Remarks by E. J. Stone.

In observing a Transit of *Venus* for the determination of Solar Parallax, there are two phases of the phenomena presented at the Ingress and Egress which should be accurately observed,—

(1.) The real internal contacts;
(2.) The apparent internal contacts.

It will, perhaps, conduce to clearness of explanation if we confine our attention to the phenomena presented at the egress. Similar phenomena are presented, in a reverse order, at the ingress.

The apparent diameter of the Sun is increased, and the apparent diameter of *Venus*, whilst on the Sun's disk, is decreased from the same causes which produce the spurious disk of a star in the focus of a lens. When the true limb of *Venus* falls in a line with the true limb of the Sun, the light is cut off, the spurious enlargement of the Sun's disk and encroachment on that of *Venus* is destroyed at the point of contact, and, as a consequence, a dark ligament appears to connect the apparent limbs of the Sun and *Venus*. This is the real internal contact.

The breadth of the connecting ligament rapidly increases, and, after some 18^s, the exact interval depending upon the circumstances of the transit and the success in first catching phase (1), the limbs of *Venus* and the Sun will appear in contact.

This is the second phase for accurate observation, the apparent internal contact. The observation of this phase is rendered more difficult by the presence of the connecting ligament. The observation, however, is of great value on account of the probable loss of phase (1) at many of the stations through atmospheric disturbance.

It will be seen from this description that the passage from the real to the apparent internal contacts is gradual. The real contacts will appear to take place instantaneously to all the observers. The description of the formation or disappearance of the connecting ligament as "instantaneous" will convey no definite information respecting the phase really observed. The required information can only be conveyed by a drawing representing, as accurately as possible, the relative breadth of the ligament, as compared with the diameter of *Venus*, when last seen at the ingress and first seen at the egress. It may be mentioned that *Venus* will appear quite within the Sun's disk at the disappearance and first appearance of the connecting ligament.

It should also be borne in mind that—if it were possible for any observer, favoured by exceptional atmospheric circumstances,

to employ extraordinary optical power and thus catch a glimpse of the connecting ligament at ingress much later and at egress much earlier than the average phase, as marked by the breadth of the ligament, caught by the rest of the observers — then such abnormal observations, however interesting for other purposes, would be absolutely injurious for the accurate determination of Solar Parallax. It is, therefore, most desirable that all the observers should employ the same optical power, which must not be fixed too high. A power of about 150 would, perhaps, meet all requirements. I would, therefore, suggest for consideration,—

1. That real internal contacts be observed.
2. That apparent internal contacts be observed.
3. That a magnifying power of about 150 be employed.
4. That all the observations should be accompanied by drawings representing, as accurately as possible, the *relative breadth of the connecting ligament as compared with the diameter of Venus*, corresponding to the time of each observation. At the real internal contact, at ingress, the breadth of the ligament when last seen should be represented.
5. Of course full notes would be given of the circumstances under which the observations were made.

On the Observation of the Transits of Venus by means of Photography. By Warren De La Rue, Esq.

The Astronomer Royal having intimated to me his intention of communicating to the Society a paper on the Transits of *Venus* of 1874 and 1882, and having invited me to make any suggestion which may have occurred to me on the means which Photography offers for their observation, I venture to lay the following statement before the Society: premising, however, that I do not suggest that photographic observations should displace eye observations; on the contrary, I think that both eye and photographic observations ought to be made.

It will be recollected that in 1860 I made photographic observations of the phenomena of totality, and also of the different phases of the eclipse of the Sun before and after that of centrality; the photographs of the totality and of partial phases were measured by means of a micrometer which I designed for that purpose,* and the results tabulated† and discussed.

From the discussion of the measurements of the partial phases, the angular position of the Moon's centre in relation to the Sun's centre and the distances of the Sun and Moon's centres were derived and compared with these elements calculated by Mr. Farley.‡

* This is described and represented in *Phil. Trans.*, 1862, pp. 373–374.
† Ib. p. 383. ‡ Ib. p. 387, Table III. columns.

On reference to these results, it will be seen that the observed and calculated positions agree remarkably well,* and that the deduced quantity for the nearest approach of the Sun and Moon's centres only differs by $\frac{9}{10}$ths of a second of arc from that calculated by Mr. Farley and 1.2 second from that calculated by Mr. Carrington.

The conditions which transits of *Venus* offer for the determination of the relative position of the Sun and planet's centres are more advantageous than those presented by solar eclipses, inasmuch that it is far more easy to measure directly the distances between the centre of the disk of the Sun and that of the image of the planet upon it, than it is to measure the distances between the peripheries of the Sun and Moon,† or the angular opening of the cusps‡ of the partially eclipsed Sun. And in transits of *Venus* any error of observation would not affect the final result nearly so much as in solar eclipses; for example, in the transits of 1874 and 1882, an error of 1″ in the measurement would, for the maximum displacement, give an error of only 0″.185 in the deduced Solar Parallax.

Moreover, it may be observed that in photographic records it is by no means important to catch exactly the phases of contact, as two photographs obtained at a sufficient interval afford the means of calculating to a great degree of refinement and of tracing the path of the planet, which, for the conditions of the problem, may be considered to be a straight line between the two positions recorded.

Nor is it in any way essential, as it is with eye observations, that favourable conditions should exist for retarding the period of contact at one station and of accelerating it at another, because the chords representing the planet's path can be derived from photographic records, with as much accuracy under what would be considered unfavourable conditions as under favourable conditions for eye observations, for the length of the chords need not be directly considered in determining the nearest approach of the Sun and planet's centres.

During the duration of the transit it would be possible in a clear state of the atmosphere to obtain a series of photographs at intervals of two or three minutes, and any or all of these would be available for comparison with the records obtained at all the stations selected.

The epoch of each photographic record is determinable with the utmost accuracy, 1st, because the time of exposure is not more than the $\frac{1}{50}$ or the $\frac{1}{100}$ of a second, and 2nd, because the instantaneous slide, as it flashes before the secondary lens, affords an audible signal§ by striking against a stop a small fraction of second after it has shut off the image of the Sun. This interval might be determined by experiment and taken into account.

* *Phil. Trans.* 1862, p. 393, Table IV.
† Ib. p. 383, Table I.
‡ Ib. p. 55, Table III.
§ Ib. p. 364.

In figures 1 and 2 is represented the solar disk of the dimensions of the photographs which would be given by the Kew Photoheliograph at the epochs of 1874 and 1882, under the same conditions of adjustment as existed during the eclipse observations of 1860; on this occasion $\frac{1}{1000}$th of an inch represented 0″·496 second of arc;* the images are direct, as given by that instrument.

The following data, part of which are derived from Mr. Hind's paper in the *Comptes Rendus* for July 22, 1861, on the circumstances of the Transits of *Venus* for 1874 and 1882, have been used in setting out the diagrams.

1874.

⊙ Semi-diameter (Le Verrier)	16′ 14″·98
⊙ Inclination towards East of Axis to a Meridian drawn through Sun's centre	12° 49′
Heliographic Latitude of Earth S.	22′
♀ Semi-diameter	31″·41
Ingress, Ext. Contact 49°·4 N. towards E.	
Egress, Ext. Contact 20° N. towards W.	
♀ Horizontal Parallax	32″·44
⊙ ,, ,, (by Encke)	8″·71

1882.

⊙ Semi-diameter (Le Verrier)	16′ 14″·64
⊙ Inclination towards East of Axis to a Meridian drawn through Sun's centre	13° 52′
Heliographic Latitude of Earth N.	12′
♀ Semi-diameter	31″·40
Ingress, Ext. Contact 145°·3 N. towards East	
Egress, Ext. Contact 113°·9 N. towards West	
♀ Horizontal Parallax	32″·43
⊙ ,, ,, (by Encke)	8″·71

In the Kew Photoheliograph the solar disk would, at the epoch of the transit of 1874, have a semi-diameter of 1965·8 thousandths of an inch (a diameter of nearly four inches), *Venus* a semi-diameter of 63·33 of these units, and the parallax of *Venus* referred to the Sun would be represented by 47·85 of the units; the maximum possible displacement being 95·7 units, nearly $\frac{1}{10}$th of an inch. In 1882 the Sun's semi-diameter wou

* *Phil. Trans.* 1862, p. 383, Table I.

be 1964·9 units; that of *Venus* 63·31 units; the parallax of *Venus* referred to the Sun 47·82 units; the maximum possible displacement 95·6 units.

When the photographs have been secured, the measurements by means of the micrometer, which would have to be performed, consist in determinations of the Sun's semi-diameter, in units of the arbitrary scale of thousandths of an inch, the angle of position of *a*, *b*, *d*, &c., and the distances *a c*, *b c*, *d c*, figs. 1 and 2; with these data the portion of the chord *a b*, *b d*, &c., could be calculated. The measurements by means of the micrometer described in the *Phil. Trans.* 1862, pp. 373-374, can be obtained to the $\frac{1}{2000}$th of an inch (0″·25), and the position angles to one or two seconds of arc. For each photograph measurements made at different times are remarkably accordant; the greatest difference between the semi-diameter of the Sun of the several eclipse pictures of 1860 was $\frac{9}{1000}$ths of an inch, or about 4″·5; but, on taking the mean of measurements of forty-five photographs by two different methods, the difference was only $\frac{1\cdot5}{1000}$, or about 0″·75. I am inclined to believe that the distance *a b*, *a c*, could be ascertained to within 1″, by means of a few pictures, and possibly to 0″·25, if a sufficient number of photographs were obtained.

Since 1860 means have been devised of ascertaining the optical distortion of the photographic pictures, and, in a paper* recently sent in to the Royal Society by myself, Mr. Balfour Stewart, and Mr. Loewy, this has been treated of; if this optical distortion had been ascertained for the photographs of the eclipse, 1860,† it is certain that even more accurate results would have been obtained, because a part of the small discrepancies in the measurements arose out of the circumstance of the centre of some of the pictures not being in the optical axis of the instrument. In the eclipse pictures the whole disk of the Sun was not, however, visible, but in the transit pictures the whole disks of the Sun and *Venus* would be seen, and the determination of the requisite data, consequently, far more accurate. By photographing a suitable scale of equal parts placed at about a mile or two miles from the heliograph, a picture would be obtained in which different equal portions of the scale would occupy different lengths; by measuring the divisions by means of the micrometer, the effect of optical distortion could be ascertained to a great nicety.

Such a scale of equal parts, if erected at a distance of two miles, would have to be about 110 feet long, in order that its image might be somewhat longer than that of the diameter of the Sun picture. It could be made by fixing two horizontal rails of

* Now being printed in the *Phil. Trans.*

† It is not possible to do so now on account of the relative adjustment of the object-glass and secondary lens having been disturbed after the observations had been made.

wood, one over the other, on vertical posts, so that their outer edges would be distant about 3 feet. On these horizontal rails plates of zinc, 2 feet wide and 3 feet long, would have to be fastened so as to leave an interval of exactly 2 feet between two adjacent plates. These plates could all be made of exactly the same width by clamping the whole series together and planing their edges in a planing machine; in fixing them on the horizontal rails, one of the plates could be used temporarily as a template to govern the interval between a plate previously screwed down and one about to be fixed. The horizontal bars would be strengthened here and there by cross bars of wood behind the plates and by cross tension rods properly distributed. To facilitate the depiction of the scale, it should be placed on the summit of a low hill or, by preference, near the sea-shore, in order that the light of the sky might shine through the intervals between the plates.

The focus of the object-glass of the Kew Heliograph, for an object situated at a distance of two miles, would be about $\frac{8}{100}$ths of an inch longer than that for parallel rays; this elongation of the focus produces a little indistinctness in the pictures, but does not prevent measurements of them being made. Before finally fixing the position of the secondary (or enlarging) lens for Sun pictures, sharply defined pictures of the scale could be procured by adjusting the focus accurately for the scale of equal parts; subsequently other pictures of the scale could be procured after the focus had been adjusted and fixed for Sun pictures. This double set of pictures would afford all the requisite data for ascertaining the correction for optical distortion; and, the exact distance of the scale of equal parts being known, for obtaining the value in arc for the divisions of the micrometer.

Fears have been expressed that the collodion, in drying, becomes distorted; experiments, however, in 1860–61 have demonstrated that the shrinkage is only in the direction of the thickness.* But, as in the case of the Solar Parallax no refinement of correction ought to be neglected; it would be quite easy to ascertain once more whether any distortion does take place, by taking photographs on glass plates on which lines about $\frac{1}{4}$ of an inch distant had been previously etched, the collodion, which should be rendered purposely contaminated with particles in suspension, should be poured on the ruled sides to avoid parallax:—after all the operations of photography the film would have to be examined from the back, and the position of certain impurities with reference to the ruled lines noted whilst the collodion was wet, and after it had dried.

Lastly, on looking at the charts given in the May number o' 1857, and in the June number of 1864, in Mr. Airy's valuab papers, it appears that, at all events, six stations could be selec favourable for photographic records: I would propose that

* *Phil. Trans.* 1862, p. 36.



precisely similar instruments should be prepared and mounted equatoreally, but without circles, or a driving clock, as these are quite unnecessary. The optical distortion of these instruments could be determined beforehand, and no further experiment would be necessary, as all the optical parts must be rigidly fixed; it would only be necessary to make a provision for the rotation of the telescope in a cradle on the declination axis for the object of adjusting the cross wires in a position of 45° to a meridian passing through the Sun's centre; in other respects the mounting would be of the simplest kind.

No difficulties exist in photographing a transit of *Venus;* the operations are quite the same as those practised daily at the Kew Observatory; no strain on the nerves would occur, as in the anxiety consequent on the desire of rendering available every moment of the short duration of a solar eclipse. All the operations could be conducted with that calm so essential for such a problem as the determination of the Solar Parallax, and I feel confidence in recommending that timely steps should be taken to secure photographic records of the Transits of *Venus* in 1874 and 1882.

Ephemeris for Observations of the Physical Features of the Planet Mars during the Opposition of 1869. By A. Marth, Esq.

(*Communicated by R. S. Newall, Esq.*)

The following Ephemeris may probably be of service to those observers who are engaged upon observations of *Mars.* It is computed on the assumption that the axis of rotation of the planet is directed to the point R.A. 317° 51′, P.D. 39° 46′, and that the time of rotation is = $24^h\ 37^m\ 22^s{\cdot}62$ mean time. The Ephemeris gives for 8^h Greenwich Sidereal Time of every 5th day the values of—

L, the Areographic Longitude of the Apparent Centre of the Planet's Disk, reckoned from an arbitrary First Meridian to the West.

B, the Aerographic Latitude of the Centre, or the Elevation of the Earth above the plane of Mars' Equator.

P, the Angle of Position of the visible Pole at the Centre.

Q, ,, ,, ,, greatest Phase.

q, the Breadth of the greatest Phase } the Diameter of Mars at the Distance
d, the Diameter of the Disk } 1 being assumed = 9″·328.

2h Gr. Sid. T. 1869.	L.		B.	P.	Q.	r.	d.
	°	°	° ′	° ′	° ′	″	″
Jan. 1	260·0	−1·1	24 21 N.	13 56	289 4	0·66	10·89
6	209·7	−0·7	24 11	13 57	288 24	·59	11·36
11	159·8	−0·4	23 57	13 46	287 29	·50	11·83
16	110·2	0·0	23 41	13 22	286 14	·40	12·29
21	61·0	+0·3	23 22	12 44	284 26	·30	12·72
26	12·1	+0·5	23 2	11 53	281 47	·20	13·10
31	323·4	+0·8	22 40	10 51	277 23	·12	13·41
Feb. 5	275·0	+0·9	22 16	9 40	268 21	·05	13·64
10	226·7	+1·0	21 52	8 22	240 59	·01	13·76
15	178·5	+0·9	21 27	7 1	164 49	·01	13·76
20	130·2	+0·7	21 4	5 40	132 55	·05	13·66
25	81·7	+0·5	20 44	4 24	122 54	·11	13·44
Mar. 2	33·0	+0·3	20 26	3 17	118 5	·19	13·14
7	344·1	−0·1	20 14	2 20	115 9	·29	12 77
12	294·8	−0·4	20 7	1 34	113 10	·39	12·35
17	245·2	−0·7	20 6	1 2	111 48	·48	11·90
22	195·3	−1·1	20 11	0 44	110 45	·58	11·44
27	145·0	−1·4	20 21	0 39	110 1	·65	10·97
Apr. 1	94·4		20 37 N.	0 45	109 32	0·71	10·52

Increase of L for other Hours and Days.

Gr. Sid. T.	0	1	2	3	4
h	°	°	°	°	°
4	301·6	291·8	282·0	272·1	262·3
5	316·2	306·4	296·6	286·7	276·9
6	330·8	321·0	311·1	301·3	291·5
7	345·4	335·6	325·7	315·9	306·1
8	0·0	350·2	340·3	330·5	320·6
9	14·6	4·8	354·9	345·1	335·2
10	29·2	19·3	9·5	355·7	349·8
11	43·8	33·9	24·1	14·3	4·4
12	58·4	48·5	38·7	28·8	19·0
13	73·0	63·1	53·3	43·4	33·6
14	87·5	77·7	67·9	58·0	48·2
15	102·1	92·3	82·5	72·6	62·8
16	116·7	106·9	97·0	87·2	77·4

Hourly Increase, 14°·59.

This is the increase for the average difference of the successive values of L, which is = 1750°·80. The deviations from this average, which depend on the variation of the planet's apparen

motion and distance, are given above as differences between the values of L, and must be allowed for where needed.

To diminish the existing uncertainty of the direction of the axis of rotation, trustworthy measures of the angles of position of some well-defined spot near the pole with respect to the centre of the disk are required. The spring equinox for the northern hemisphere of *Mars* has occurred, 1868, Sept. 28, and the summer solution will take place, 1869, April 14. It seems, therefore, not unlikely that, during the next months, the white spot (supposed to be snow) near the pole will offer a good object for observation. But, to get rid of the assumption that its centre occupies a constant position on the ball of the planet, and of the still greater assumption that it coincides with the pole itself, it is necessary that trustworthy angles of position should not only be observed on a sufficient number of nights, but also that, if practicable, two or three sets of observations be taken each night at intervals of some hours.

Though liable to some uncertainty, the direction of the axis and the time of rotation are sufficiently well known for the purpose of placing the drawings of *Mars*, which have been made during the last ten or twenty years, in their proper order. A list of the sketches known to us, arranged in the order of the areographical longitudes and latitudes of their centres, will be communicated when it can be given with some completeness. At present, probably several series of published sketches are not yet at our disposal. Of unpublished drawings there are very likely some series which may deserve to be at least consulted. Several of them are probably in the library of the Royal Astronomical Society. Observers who desire to have their sketches inserted in the proposed list will, perhaps, consider it worth while to communicate to us (at Ferndene, Gateshead), if not copies or tracings, at least the approximate times (if possible within a few minutes) to which their drawings refer.

A statement of the values of B and P for the times of the last half-dozen oppositions may, perhaps, be of interest to some readers.

		B.	P.
		° ′	° ′
Opposition 1856	April 2	25 49 N.	33 35
58	May 15	12 4 N.	40 43
60	July 17	14 22 S.	189 26
62	Oct. 5	22 40 S.	145 17
64	Nov. 30	6 25 S.	142 40
67	Jan. 10	10 38 N.	343 54

Description of Drawings of Mars. By J. Joynson, Esq.

The accompanying sheets are lithographed copies of a series of diagrams already sent to the Society. They show in consecutive order the appearances presented by the planet during one complete rotation, at each opposition, from 1862 to 1867. They have been lithographed for the purpose of sending a copy of each to any Member of the Society or others interested who may apply for them, and at the same time undertake to send me a similar series, to be taken by the applicant during the coming opposition; as, by this means, it is hoped that something near a correct delineation of the planet during one complete rotation will be obtained. It is earnestly requested that, as far as possible, every observer will obtain the appearances during the whole rotation, and so avoid the mistake that has been made of supposing that the same appearance was presented by two different parts of the planet. It is also particularly desired that no approach to fancy sketches may be made, or any attempt permitted in the way of striking drawings; but that they be strictly confined to the appearance presented by the planet at the time noted at the foot of each drawing. The aperture of the telescope and the power used should be given.

The plan adopted by the writer was to commence the series as soon as the planet presented well-defined markings on the disk, and then make a drawing about every forty minutes. By this means four or five drawings were made; and by doing the same on the next evening, commencing about the same time, the observer would soon see that the drawings arranged themselves somewhat in this order;—

	st	nd	rd	th	th
1st Night's Drawings	1	2	3	4	5
2nd ,,	1	2	3	4	5
3rd ,,	1	2	3	4	5
4th ,,	1	2	3	4	5

That is, he would get about one aspect sooner each night. What is seen, say at 7 o'clock on the first evening, is not seen on the next until $7^h\ 37^m$, and on the following until $8^h\ 15^m$, varying about 37 minutes each succeeding night

However the drawings are originally made,—that is, with reference to the Vertex at the Pole of the Earth,—it will be desirable to place them all in the final drawing with the pole of the planet in one direction.

Waterloo, near Liverpool, October 2, 1868.

The Transit of Mercury. By John Browning, F.R.A.S.

On the morning of November 5th the Sun rose to view at my small telescope about $8^h\ 15^m$. At this time *Mercury* was at once seen as a very dark purplish-grey spot on the solar disk. The limb of the planet was ill defined, perceptibly darkest towards the centre, and surrounded by an aureola slightly brighter than the solar disk. Near the centre of the planet appeared two bright grey spots close together.

The spot to the N.W. seemed to be much the most conspicuous, and to be elongated, in the direction of the smaller spot. Possibly the faint spot was produced by the boiling of the image, which was sometimes very bad.

Mr. Pritchard has kindly informed me that the shadow of a black disk is a white spot. Possibly this may prove the explanation of these otherwise unaccountable appearances.

When the limb of the planet was still distant nearly one-third of its diameter from the Sun's limb, the limb of the planet seemed drawn out towards the Sun's limb. About $2\frac{3}{4}^s$ after this the contact seemed complete.

For this observation I used a silvered-glass reflector of $4\frac{1}{2}$-in. diameter, and $5\frac{1}{2}$ feet focus, made for me by Mr. With. The mirror had been tested up to 700 on stars. The Sun being low I did not use a solar eye-piece, but simply modified the light with a sliding wedge of dark neutral tint-glass corrected for refraction.

Of the three drawings I have the honour of exhibiting before the Society the first represents the planet and shows the aureola and the two white spots. The other two show the appearance of the planet at the times of real and apparent contact with the Sun's limb.

As I did not see either the aureola or the white spots towards the time of contact, I have omitted them in the figures which represent the contact.

The Transit of Mercury. By C. G. Talmage, Esq.

The transit of *Mercury* was well seen here. At sunrise the sky was perfectly cloudless, and remained so until nearly noon. When I first directed the telescope to the Sun at $19^h\ 50^m$ M.T. the limbs were boiling violently, but by $20^h\ 30^m$ the definition was greatly improved. From the first the limbs of *Mercury* were much better defined than those of the Sun. I watched very carefully for any apparent distortion of the limbs of *Mercury*, but saw none. The time of internal first contact I consider better than the time of last contact.

	Egress.	
	h m	h m
First contact	21 0	12 38
Last contact	21 3	5 70

I used the full aperture of 10 inches, with a diagonal eye-piece. Power 200.

Mr. J. G. Barclay's Observatory,
Leyton, Essex.

The Transit of Mercury. By G. Knott, Esq.

Although the morning of November 5th was fine, definition in the telescope was so disturbed that the transit of *Mercury* was not well observed at this station. With an aperture of 2¾-in. and a negative eye-piece magnifying 89 times, I observed the egress as follows,—

	h m s	
First contact, Nov. 4	21 0 25·9	G.M.T.
Last contact	21 2 32·6	

both results being very uncertain. With the full aperture, 7½-in. and a Dawes' solar eye-piece with powers 95 and 145, I carefully scrutinised the planet during intervals of tolerable definition. At times I fancied that I saw traces of a narrow penumbra surrounding the body of the planet, but when best seen the disk appeared to be uniformly black, and the bright spot described by some observers, though looked for, was not seen. In a transient interval of pretty steady definition some few seconds before first contact, I caught a glimpse of a fine black ligature connecting the planet with the Sun's limb. The "boiling" of the limb was so troublesome that the observed times of contact may be some seconds in error.

Woodcroft Observatory, Cuckfield,
December 10, 1868.

Description of a new Chronograph Pen. By R. L. J. Ellery, Esq., Director of the Observatory of Melbourne.

The drawing, which gives the full size of the pen, is in the woodcut reduced to the scale of one-third.

Fig. 1. *a* is the reservoir of glass, on which a brass cover, *b*, *b'*, is cemented. The syphon, *e*, *e*, *e'*, is made of spirit thermometer tube, about 0·2 inch diameter, with a bore of 0·05 inch, and is bent as shown, so that one leg reaches nearly to the bottom of the reservoir; it is fastened to the cover by a joint like a gas-union, one part of which is cemented to the tube, another part is soldered into the cover, and the clamp screw M draws the two parts together air-tight; the length of the tube will depend on the size of the barrel, distance of electro-magnet, &c. At *e'* the tube is bent at a convenient angle, and the end ground square,

on this end a tube carrying a screw-thread is cemented, into which the nib is clamped as shown at *n n*. To support the tube an arm, *h h*, of thin brass projects from the reservoir cover, and ends in a notch at *o*, in which the tube lies, and in which it is firmly retained by means of the collar and the screw, *f*, *g*. The air tube, *c d*, is a piece of tube like the syphon, but reaching to

Fig 1

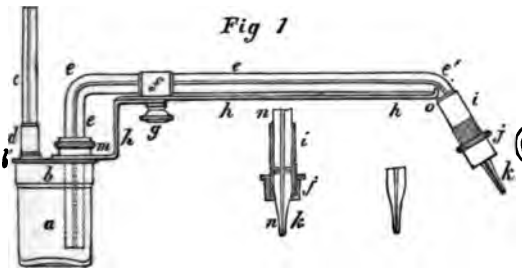
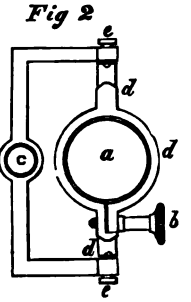


Fig 2



just within the cover. The cover *b'* itself unscrews from the collar *b*, which is permanently cemented to the reservoir. The nib is made of same sized tube as the syphon by drawing out in a blow-pipe flame ; some skill, however, is required to reduce the bore gradually towards the point, and at the same time retain sufficient glass around it. The nibs are ground square at the thick end, and cut off to the required length or to the required fineness of bore, and then ground to a rounded point on a stone or with fine emery, and slightly polished on Arkansas stone or with rouge to take away any asperities likely to catch the fibres of paper. Sketch *n n* shows how it is fitted on ; a perforated washer of sheet India rubber being placed between the ends of the tube and nib. The fluid used is a strong solution of Judson's solid aniline blue in boiling distilled water. To fill the pen the cover is unscrewed, or *d* taken out, and the requisite quantity of fluid put in; screwing the cover on air-tight and blowing into the air-tube C forces the ink through the syphon into the nib, from whence it oozes in a drop. The drop being removed and the pen adjusted in a suitable holder on the armature lever of the electro magnet, with the nib resting on the revolving barrel, the ink will flow as freely or as slowly as required, by altering the "head," which is readily done by lowering or raising the reservoir in the clip or holder, of course the pressure of the pen on the barrel or paper must be regulated by suitable springs or counterpoises.

The advantages of this pen are that any sediment that may occur in the fluid falls to the bottom of the reservoir, and is never likely to reach the fine bore of the nib. When once filled it can be used day after day till empty; the flow, if stopped by drying at the nib, can be usually re-established by rubbing with the moistened finger, or, at the worst, by removal of the nib. The flow can be regulated by raising or lowering the reservoir in its

clip, and very fine or very strong lines obtained by using nibs with finer or coarser bores.

Fig. 2 represents the arrangement by which the pen is attached to the electro-magnet armature. C is the armature axis; *d d d*, the pen-holder suspended between two centre screws *e e*.

The reservoir is clamped in the ring *a*, which is lined with a piece of leather and clamped by screw *b*.

Observations of the Meteoric Shower of November 13–14, 1868, *made at the Glasgow Observatory.* By Prof. Grant.

During the earlier part of the night the sky was so overspread with dense vapours that nothing could be seen. About $15^h\ 30^m$ the clouds commenced slowly to disperse, and through the openings between them a meteor might now and then be seen shooting across the heavens. The state of the sky continued to improve till about $16^h\ 30^m$, when it finally became pretty clear in all directions. It now became evident that a shower of meteors was in progress ; nor did it require long time to be convinced that the phenomenon was no other than an apparition of the great November shower which had been so conspicuous during the two or three previous years. In every instance the course of the meteors was found, when traced back, to emanate from the constellation *Leo*, the radiant appearing to coincide very nearly with the middle point of a line joining γ and ι of that constellation. At $16^h\ 20^m$ the number of meteors visible amounted to two or three in a minute. In their general features they presented no essential difference from the meteors observed on the occasion of the memorable apparition of November 13–14, 1866. Their colour was white, mingled in some instance with a slight tinge of red. They generally were accompanied by trains which lasted two or three seconds. I failed, however, to discover any trace of the beautiful green which formed so interesting a feature of many of the meteors of November 1866. So long as they were attached to the meteors the trains appeared white, but on breaking up they assumed a greyish aspect slightly tinged with red. In three or four instances I remarked that the *débris* of the train continued visible in the heavens for two or three minutes after the disappearance of the meteor. Some of the meteors were of considerable size, three or four of them far exceeding *Jupiter* in brightness, but not equalling the planet *Venus*, which was shining with intense brilliancy in the east, and formed an excellent standard of comparison for estimating the brightness of the larger meteors. It was interesting to note the excessively short trains of the meteors in the immediate vicinity of the radiant point. I failed, however, to perceive any instance of a meteor which appeared absolutely motionless during the time of its visibility.

The shower continued sensibly to increase from $16^h\ 30^m$ till $16^h\ 56^m$, and as it appeared very desirable to endeavour to ascer-

tain the time of its maximum, I now proceeded, in conjunction with Mr. John McKinnel, the junior assistant, to count the number of meteors which might become subsequently visible. The results are given underneath. It would appear from them that the maximum of the shower occurred at $17^h\ 15^m$.

Shortly after 18^h it became cloudy, and we ceased to obtain any further observations of the phenomenon, but although the greatest intensity of the shower seemed to have already passed, it was very evident that it had not yet wholly expended itself.

Time. G.M.T.				Number of Meteors Counted.
h	m	h	m	
16	56 to	16	57	1
16	57	16	58	2
16	58	16	59	6
16	59	17	0	8
17	0	17	1	5
17	1	17	2	5
17	2	17	3	6
17	3	17	4	2
17	4	17	5	7
17	5	17	6	8
17	6	17	7	7
17	7	17	8	5
17	8	17	9	2
17	9	17	10	4
17	10	17	11	9
17	11	17	12	6
17	12	17	13	11
17	13	17	14	4
17	14	17	15	3
17	15	17	16	3
17	16	17	17	3
17	17	17	18	5
17	18	17	19	2
17	19	17	20	1
17	20	17	21	5
17	21	17	22	4
17	22	17	23	6
17	23	17	24	4
17	24	17	25	3
17	25	17	26	3
17	26	17	27	7
17	27	17	28	3
17	28	17	29	5
17	29	17	30	3
17	30	17	31	3

Time. G.T.M.				Number of Meteors Counted.
h	m	h	m	
17	31 to	17	32	2
17	32	17	33	3
17	33	17	32	4
17	34	17	35	5
17	35	17	36	4
17	36	17	37	1
17	37	17	38	0
17	38	17	39	4
17	39	17	40	4
17	40	17	41	3
17	41	17	42	4
17	42	17	43	5
17	43	17	44	3
17	44	17	45	1
17	45	17	46	1
17	46	17	47	2
17	47	17	48	3
17	48	17	49	1
17	49	17	50	3
17	50	17	51	2
17	51	17	52	2
17	52	17	53	2
17	53	17	54	2
17	54	17	55	3
17	55	17	56	4
17	56	17	57	5
17	57	17	58	0
17	58	17	59	1
17	59	18	0	0
18	0	18	1	2
18	1	18	2	6
18	2	18	3	4
18	3	18	4	3
18	4	18	5	2
18	5	18	6	4

It appears from these results that 256 meteors were counted during the interval of one hour and ten minutes included between $16^h\ 56^m$ and $18^h\ 6^m$. There can be no doubt, however, that the number really visible was considerably greater than this. It was certainly twice, and in all probability was three times as great. We may, therefore, infer that during the interval in question as many as seven or eight hundred meteors were visible.

In order to obtain a clearer view of the time of maximum I have arranged in groups of ten minutes the interval during which the meteors were counted, and have inserted the number of meteors counted in each of the subdivided intervals thus:—

			Meteors Counted.
From	16 h 56 m	to 17 h 6 m	50
	17 6	17 16	54
	17 16	17 26	36
	17 26	17 36	39
	17 36	17 46	26
	17 46	17 56	24
	17 56	18 6	27

It would appear from a comparison of these results and the general observations of the phenomenon with the corresponding observations of 1866 that the shower was much inferior in density to that of the great November shower of 1866. We have reason, however, to believe that it was characterised by a more uniform structure.

The Observatory,
Glasgow, Dec. 10, 1868.

Meteors of November 13–14, 1868. By Prof. D. Kirkwood.

The shower of November meteors has been this year unexpectedly brilliant. Unfortunately I was unable to watch the display; but during three hours on the morning of the 13th, Prof. T. A. Wylie, D.D., observed 165. A considerable proportion of these were unconformable. The number, however, radiating from *Leo* was sufficient to indicate the commencement of the shower. Remembering that Humboldt, in 1799, had seen a number of meteors after sunrise, I took a position at 8 o'clock A.M., on the west side of a building (in the shade) and watched the vicinity of the Radiant, in the hope of observing some of the largest meteors. Glimpses of five or six were, as I thought, actually obtained, when, in order to guard against optical deception, I called Mr. Allison Maxwell, a tutor in the State University, who watched about fifteen minutes. This gentleman re-

ported that he saw *one* beyond doubt, and three others less certainly. I mention the case as being the first, since the commencement of the present century, in which meteors of the November stream have been (probably) seen in the daytime.

On the night of the 13th, a committee of the senior class in the University kept watch from 11 o'clock P.M., till 4.15 A.M., when, having counted 2500, they adjourned. The maximum was about 3.30; 900 meteors having been counted during the 45 minutes immediately preceding. Many of the meteors were very brilliant, leaving long trains, some of which continued visible several minutes. Three or four were observed to explode, or separate into several fragments. No sound, however, was in any case perceived.

At $4^h 55^m$ (40 minutes after the committee at the University adjourned) Prof. Wylie commenced observing at his residence, and continued his watch till $6^h 11^m$, counting 780 in $1^h 16^m$. The whole number therefore actually counted in $6^h 31^m$ was 3280. A portion of the heavens was at times overcast with clouds; especially was this the case shortly after 4 o'clock, when the committee of students adjourned.

At frequent intervals throughout the night a lull occurred in the display; while at other times for a few seconds the meteors were so numerous that they could scarcely be counted.

A remarkable feature in the shower of the present year is its duration. As seen in Europe in 1866, and in this country in 1867, the display was limited to three or four hours. On the present occasion, however, it commenced on the night of the 12th, and had not ceased at daylight on the morning of the 14th. This would indicate considerable irregularity in the thickness of the stream.

Bloomington, Indiana, Nov. 14, 1868.

The Lunar Crater Linné. By W. R. Birt, Esq.

Having been favoured by Mr. Huggins, Capt. Noble, and Herr Von Mädler, with drawings of *Linné* as it appeared on June 26th, Aug. 26th, and Oct. 22nd, 1868, which in many respects perfectly agree with views which I obtained on the 26th of June with the Crossley Equatorial of 7·3-inch aperture, and on the 22nd of Oct. with Mr. Barclay's 10-inch refractor, I have endeavoured from them to construct an approximate section of *Linné* as it existed on Aug. 26. The drawing of this date was made by Tacchini of Palermo, and kindly forwarded to me by Herr Von Mädler. The attempt to construct a section of *Linné* must be considered as exceedingly imperfect, nevertheless it may be of assistance in contributing to more exact observation and measurement at critical periods in the appearance of the crater.

Two facts appear to have been well established lately, viz., the conical form of the object with its shadow, and the early disappearance of the cone and shadow coincident with the appearance of the white spot. The shortness of the shadow in Capt. Noble's drawing, combined with the Sun's altitude at *Linné*, indicates, I apprehend, that the cone has a very slight elevation above the surface. Assuming both drawings to be moderately accurate as to the proportion of western slope and length of shadow, a simple calculation shows that, taking the length of shadow in Mr. Huggins' drawing = 1·000, the height of the highest part of the cone was on the 26th of June, 0·014, Sun's altitude = 0°46′·5. By measurement *on the drawings* the shadows were as follows, Huggins ·21, Noble ·17 parts of ½ an inch. Tacchini's drawings do not exhibit exterior shadows. Noble's when reduced in the proportion of the drawings = ·12. Now, assuming *no* error in the drawings, nor alteration in the height of the cone, we ought to have had on the 22nd of October with a solar altitude of 1° 7′·69226 as the length of the shadow, that of June 26th being equal to 1·00000. Upon comparing the shadows (reduced to the same scale) that on October 22nd falls short of what it should have been by ·10199, or slightly more than a tenth part. At present it is quite out of our power to decide upon this difference as resulting from errors in the drawings, or measurements from them, or from change in the height of the crater, but it indicates the course we should pursue, viz. to observe the times of disappearance and reappearance of the shadow, also those of the appearance and disappearance of the white spot.

The combination of the drawings by Messrs. Huggins, Noble, and Tacchini, appears to elucidate in some degree the phenomena of the white spot. As soon as the Sun attains an elevation above the horizon equal to the inclination of the eastern slope of the cone, the shadow disappears. This was observed on June 26th, but no observation of the disappearance of the shadow on October 22nd has come to hand. We cannot, therefore, construct a section for October 22nd. Taking in the drawing of Mr. Huggins the length of the shadow = 1·00000, and the Sun's altitude at disappearance of shadow = 1° ± we can find the extent of that part of the base of the cone from the perpendicular passing through the west peak of the small crater to the extremity of the east slope = ·77497. For the determination of the inclination of the west slope we need the altitude of the Sun at the first appearance of the shadow to the west. Most observers agree that the small crater is west of the centre of the white spot. It is therefore probable that the west slope has a greater inclination than the east, and constructing the section to exhibit this we have ·77497 from the west peak to the east base, + ·36000 from the west peak to the west base, = 1·13497 for the approximate diameter of the base. This cannot be received as *accurate* from the absence of data having reference to the west slope. Tacchini says, that according to his measures the diameter of the white spot, including the declivity, was in

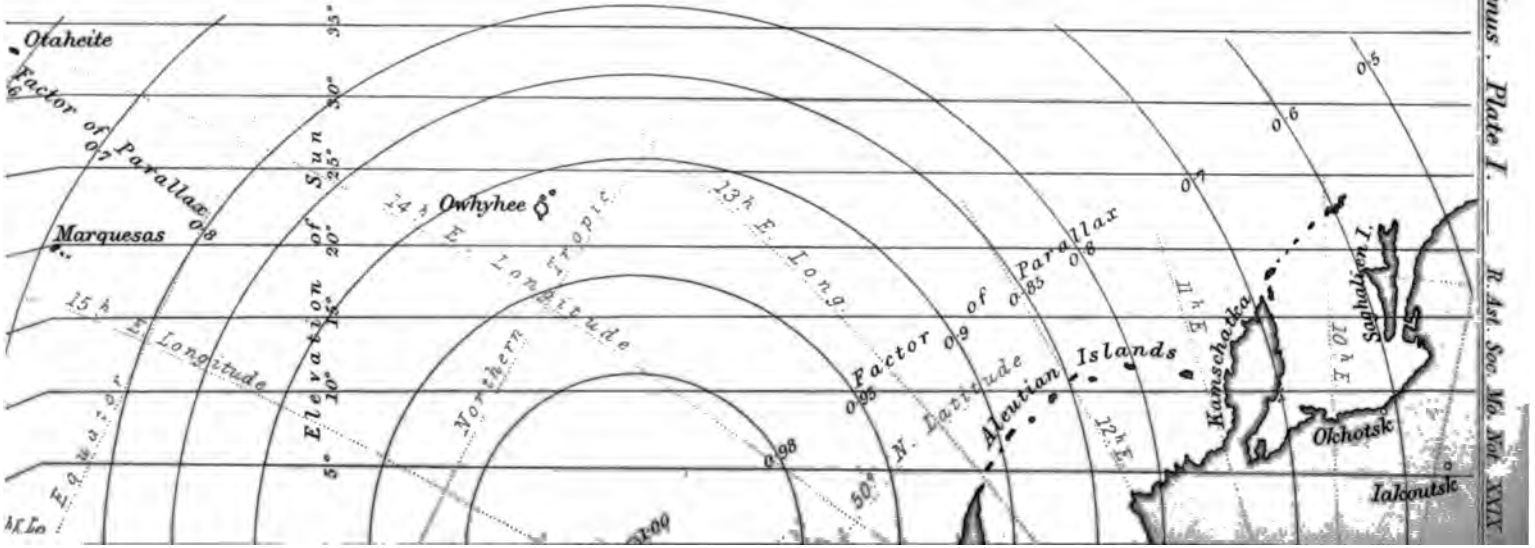


Transit of Venus, 1874, Dec.r 8.

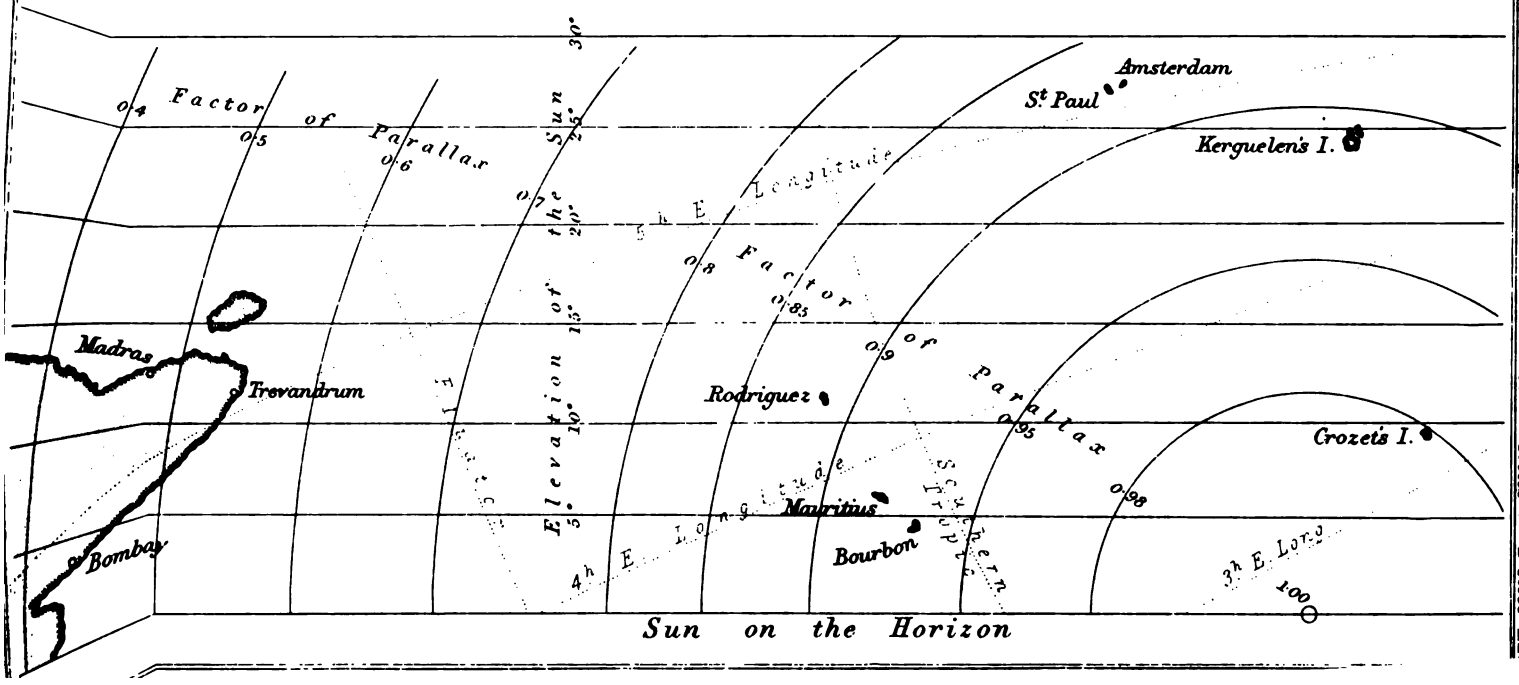
Stations for observation of the Ingress of Venus.

The Sun descending. Venus entering on the Upper Limb.

Ingress accelerated by Parallax.



Stations for observation of the Ingress of Venus.
The Sun ascending. Venus entering on the Lower Limb.
Ingress retarded by Parallax.

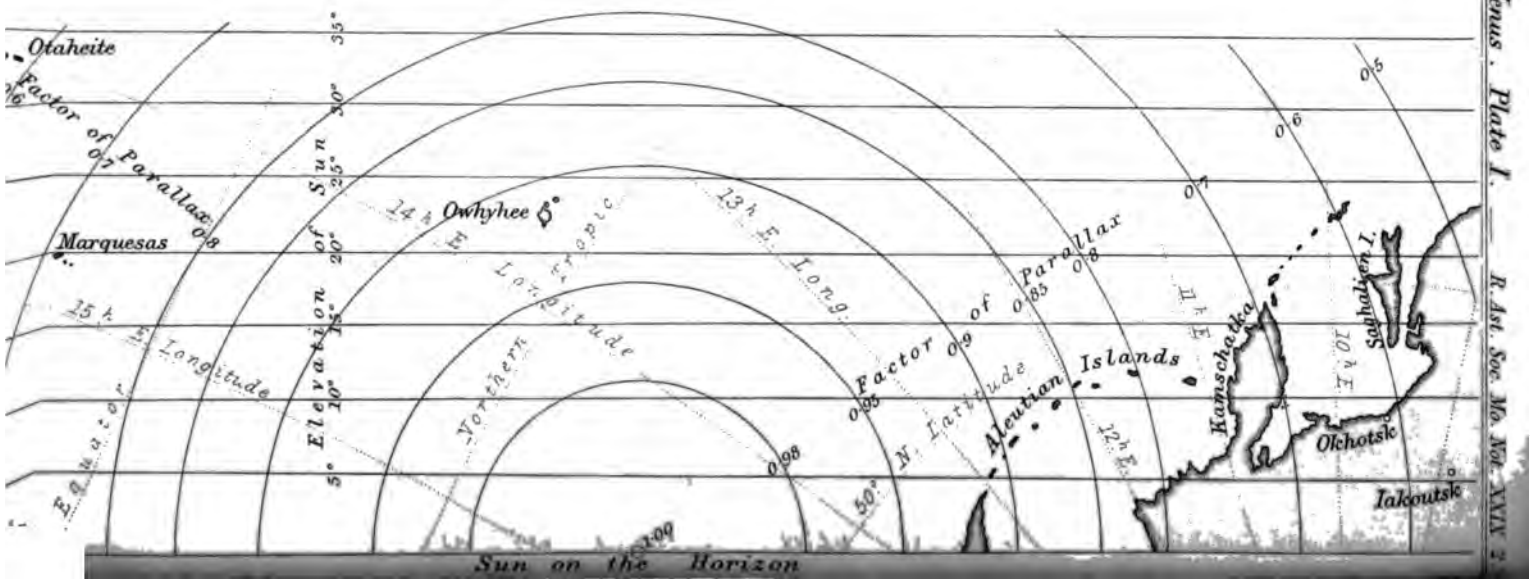


Transit of Venus, 1874, Dec.r 8.

Stations for observation of the Ingress of Venus.

The Sun descending. Venus entering on the Upper Limb.

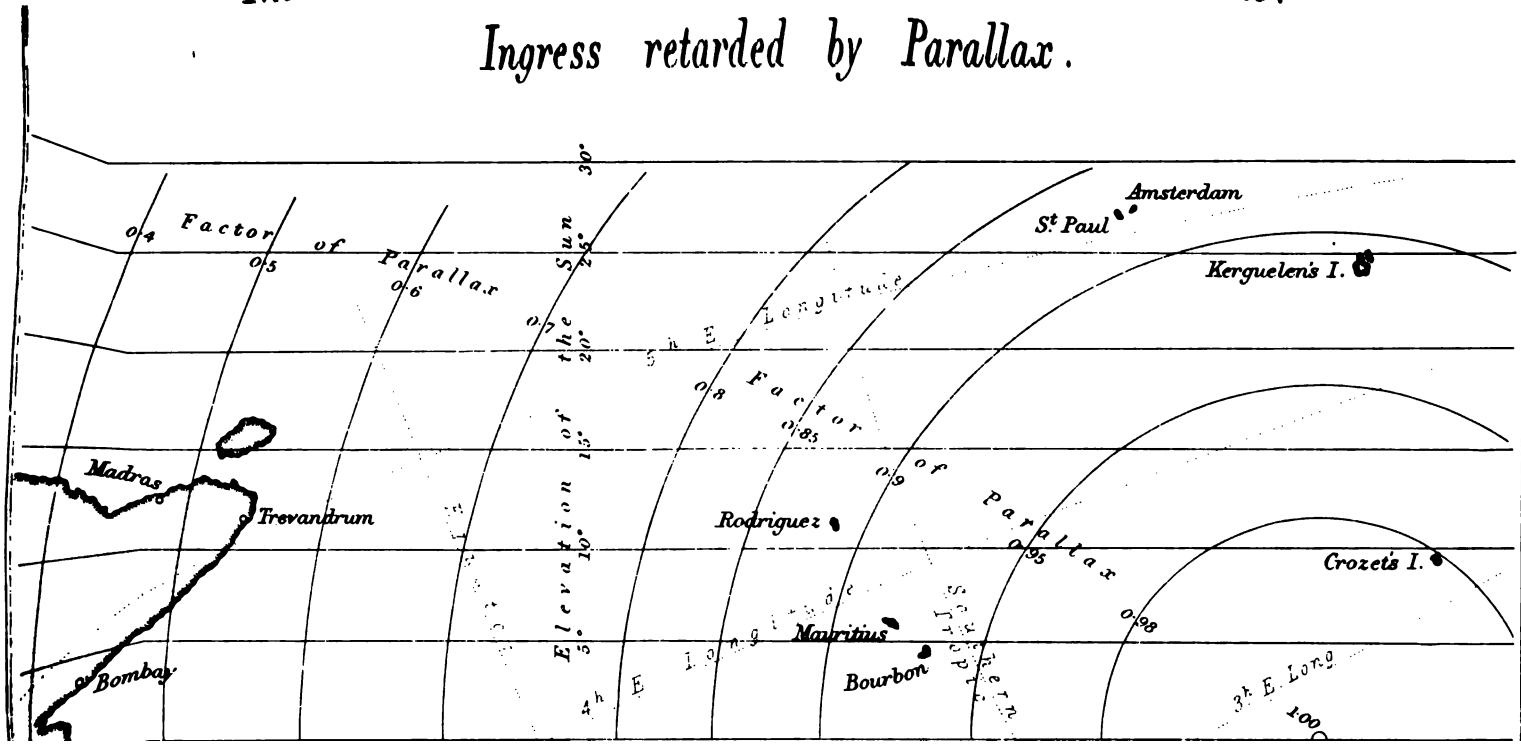
Ingress accelerated by Parallax.



Stations for observation of the Ingress of Venus.

The Sun ascending. Venus entering on the Lower Limb.

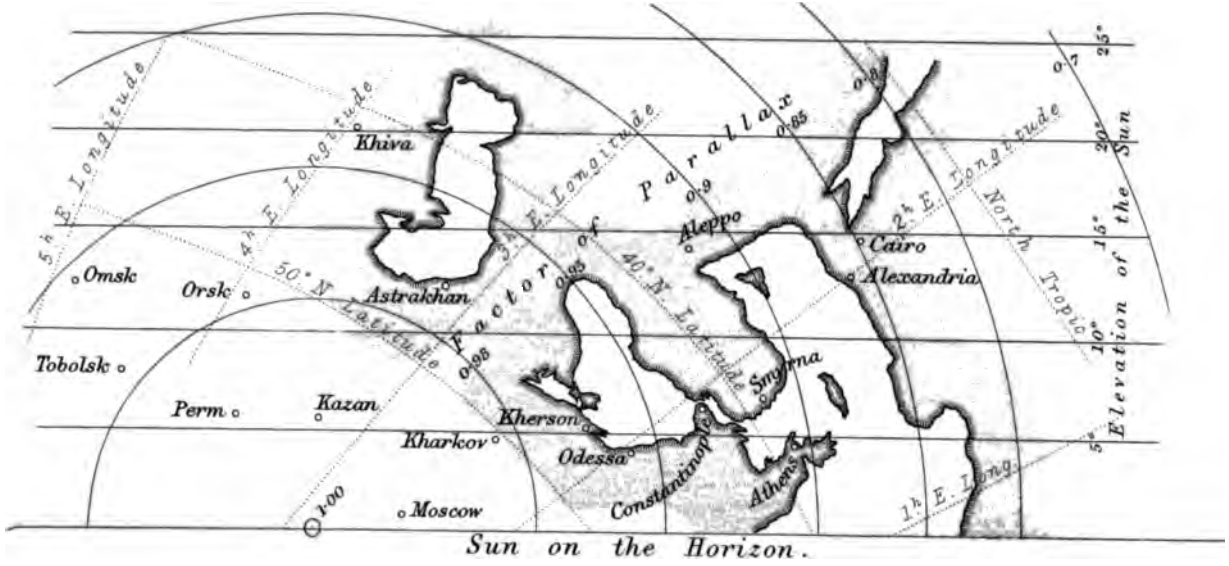
Ingress retarded by Parallax.

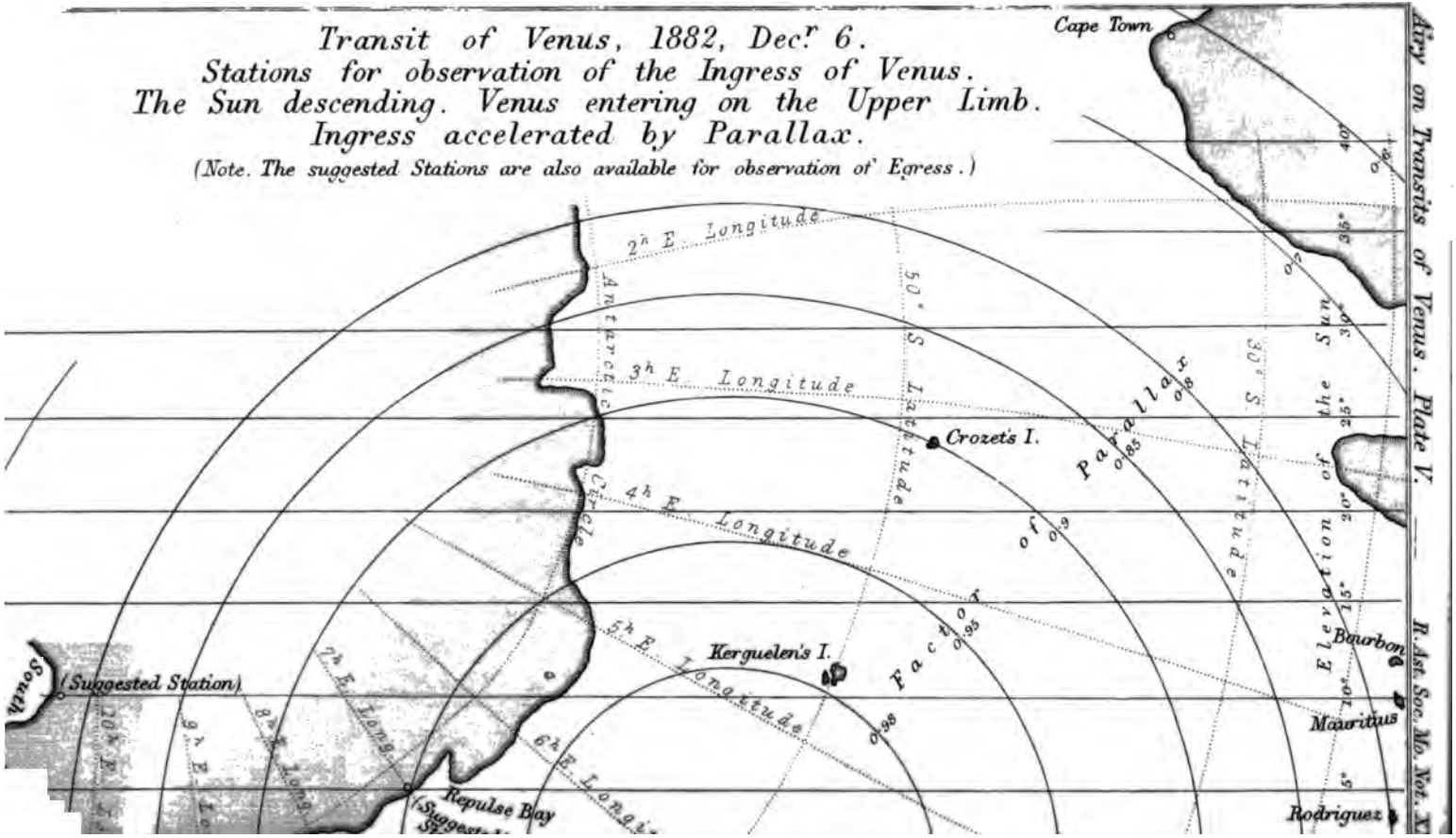


Stations for observation of the Egress of Venus.

The Sun ascending. Venus leaving the Upper Limb.

Egress retarded by Parallax.



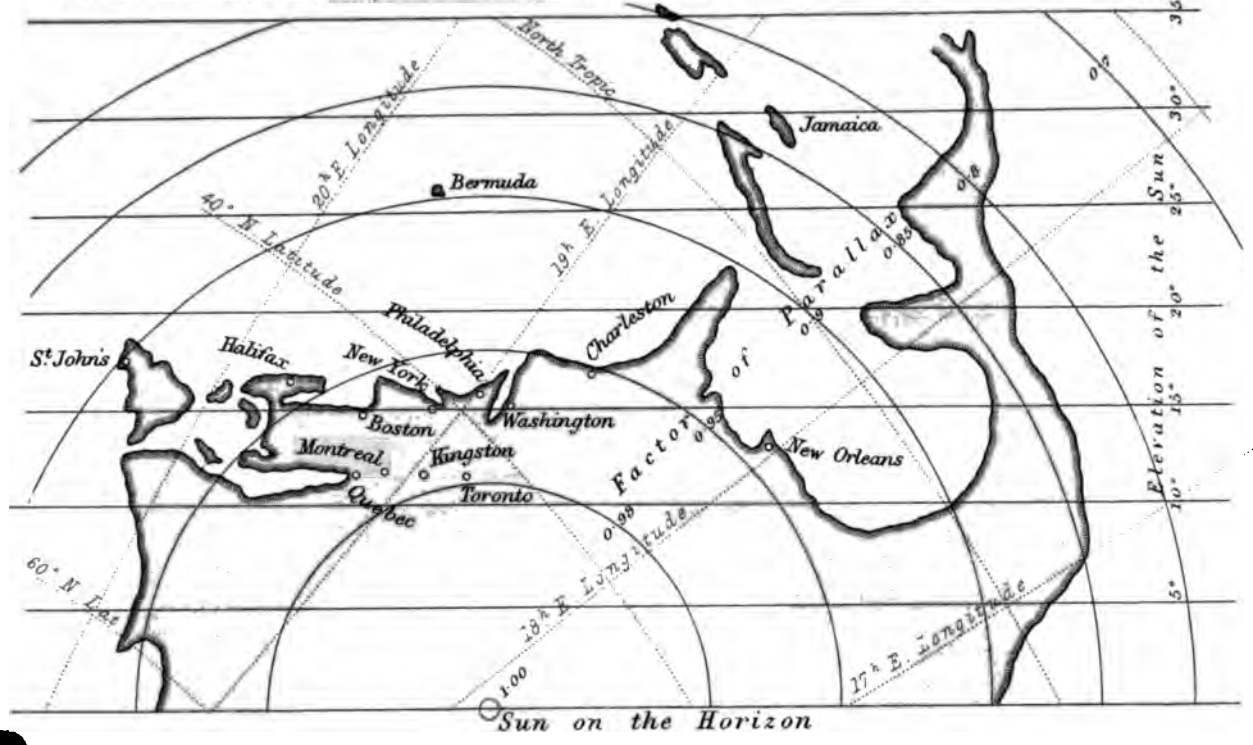

Transit of Venus, 1882, Dec.r 6.
Stations for observation of the Ingress of Venus.
The Sun descending. Venus entering on the Upper Limb.
Ingress accelerated by Parallax.
(Note. The suggested Stations are also available for observation of Egress.)
Cape Town
2h E. Longitude
3h E Longitude
4h E. Longitude
5h E Longitude
6h E Longitude
7h E Long
8h E Long
9h E Lo
Antarctic Circle
50° S Latitude
30° S Latitude
Crozet's I.
Kerguelen's I.
Parallax
Factor
0·9
0·95
0·98
0·85
0·8
Elevation of the Sun
40°
35°
30°
25°
20°
15°
10°
5°
0
Bourbon
Mauritius
Rodriguez
Repulse Bay
(Suggested Station)
South

Stations for observation of the Ingress of Venus.

The Sun ascending. Venus entering on the Lower Limb.

Ingress retarded by Parallax.

(Note. The American Stations (except Newfoundland), and the Islands, are also available for observation of Egress.)

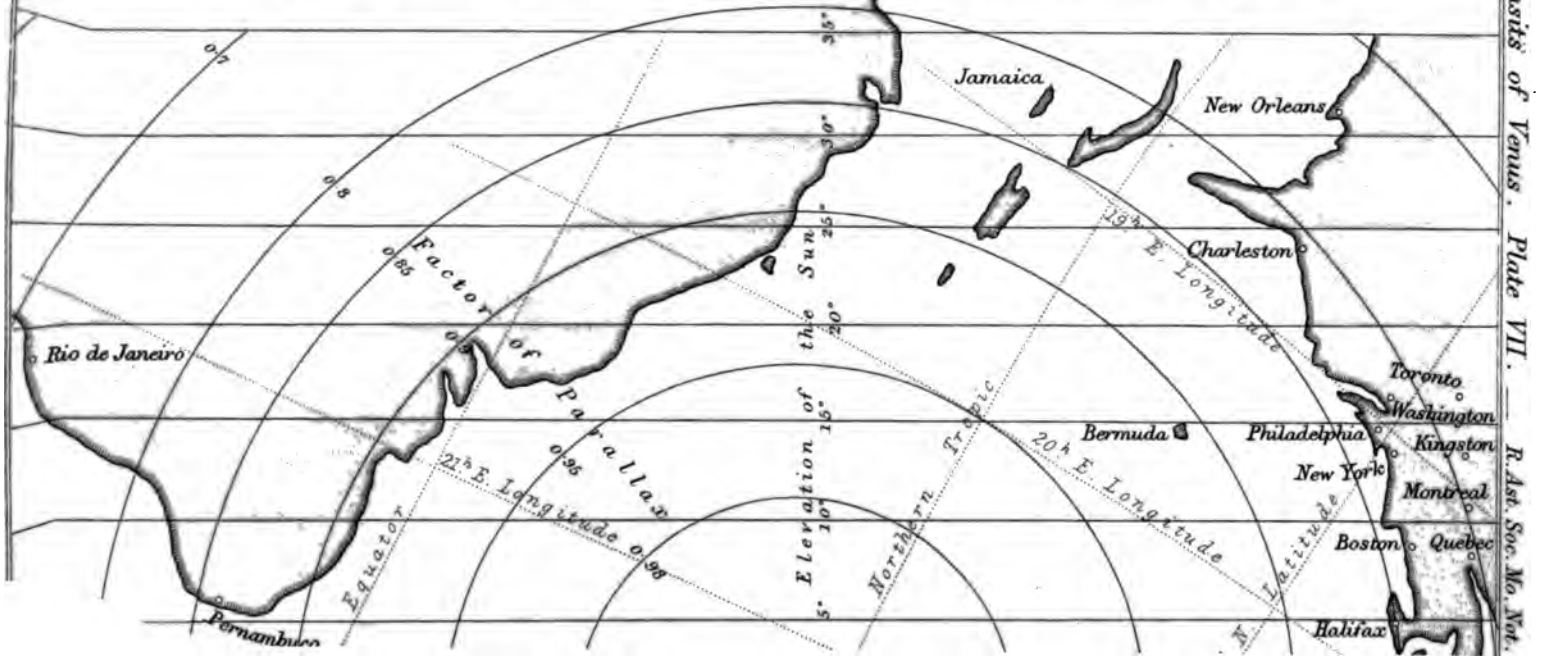


Transit of Venus, 1882, Dec.r 6.

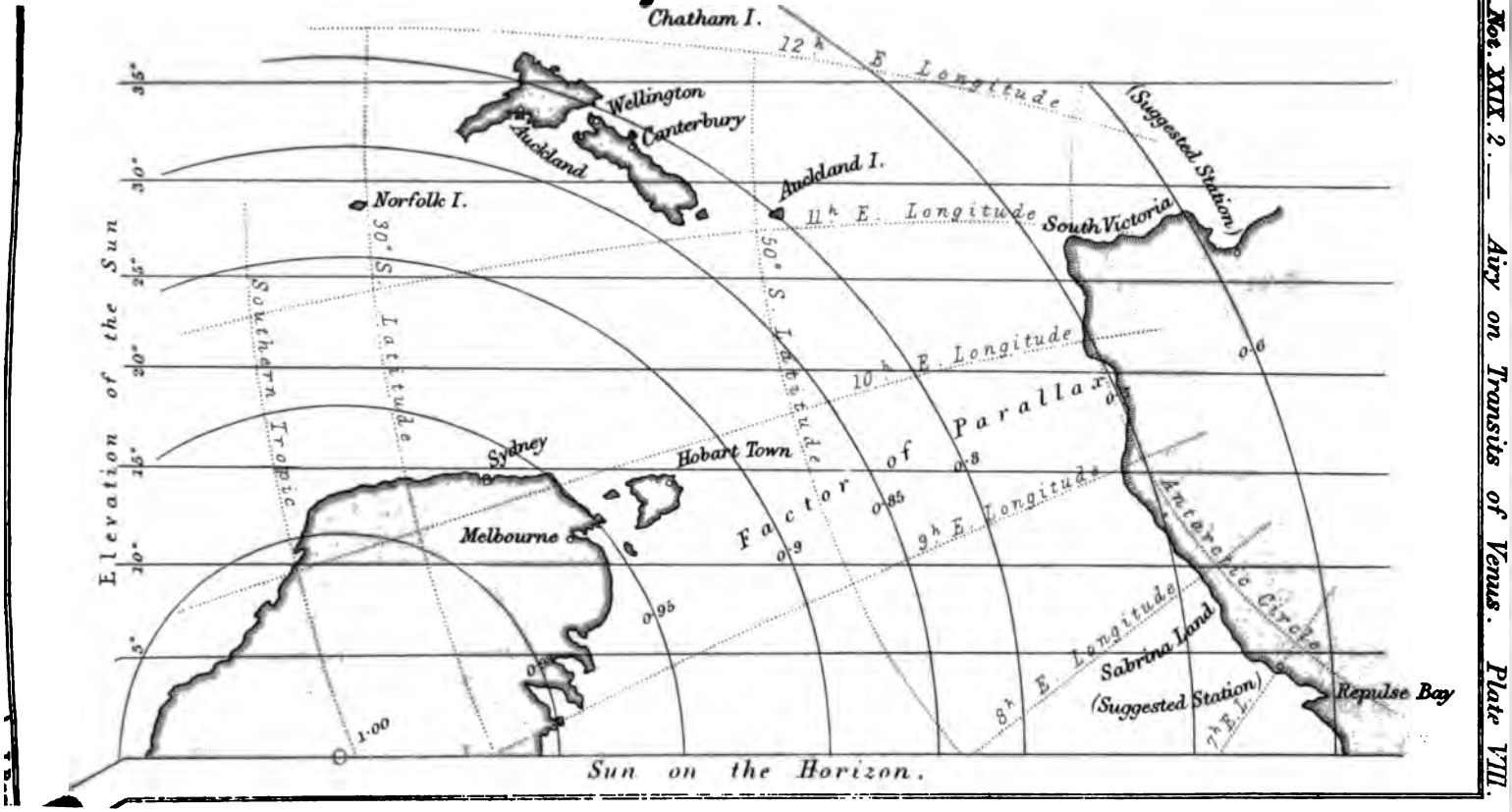
Stations for observation of the Egress of Venus.

The Sun descending. Venus leaving the Lower Limb. Egress accelerated by Parallax.

(Note. The North American Stations and the Islands are also available for observation of Ingress.)



(Note. The 'Suggested Stations' are also available for observation of Ingress.)



Mem. XXIX. 2.—Airy on Transits of Venus. Plate VIII.



Figure 1.
1874.

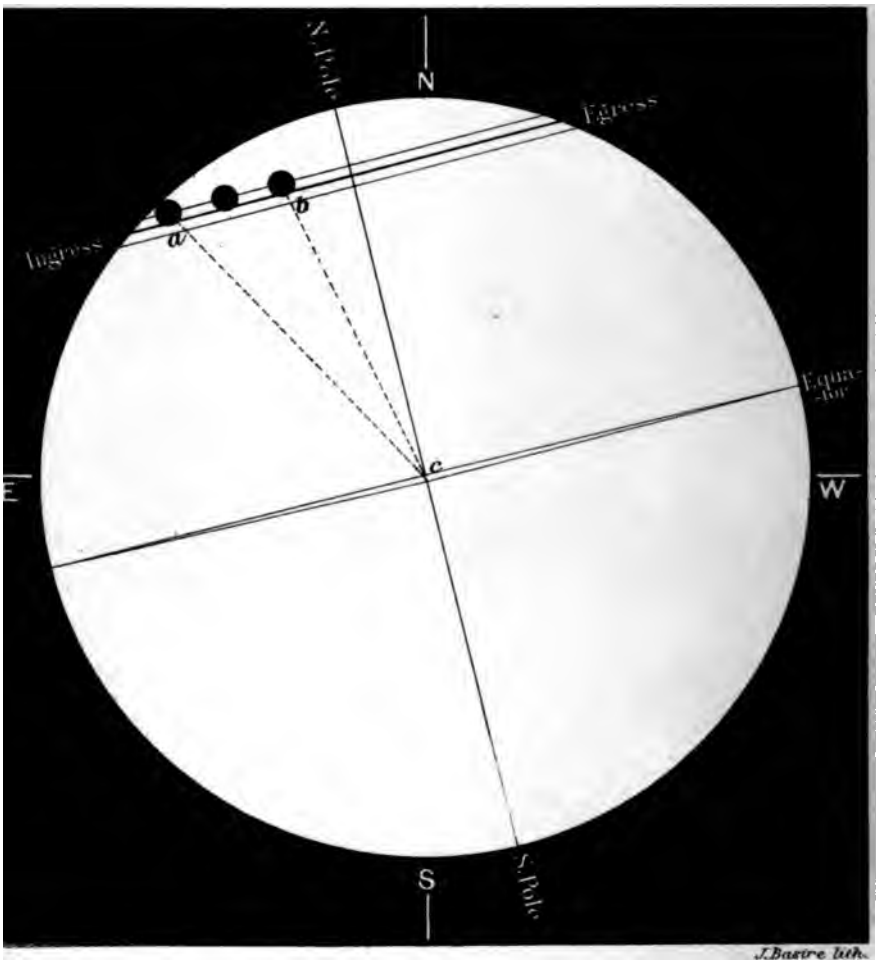
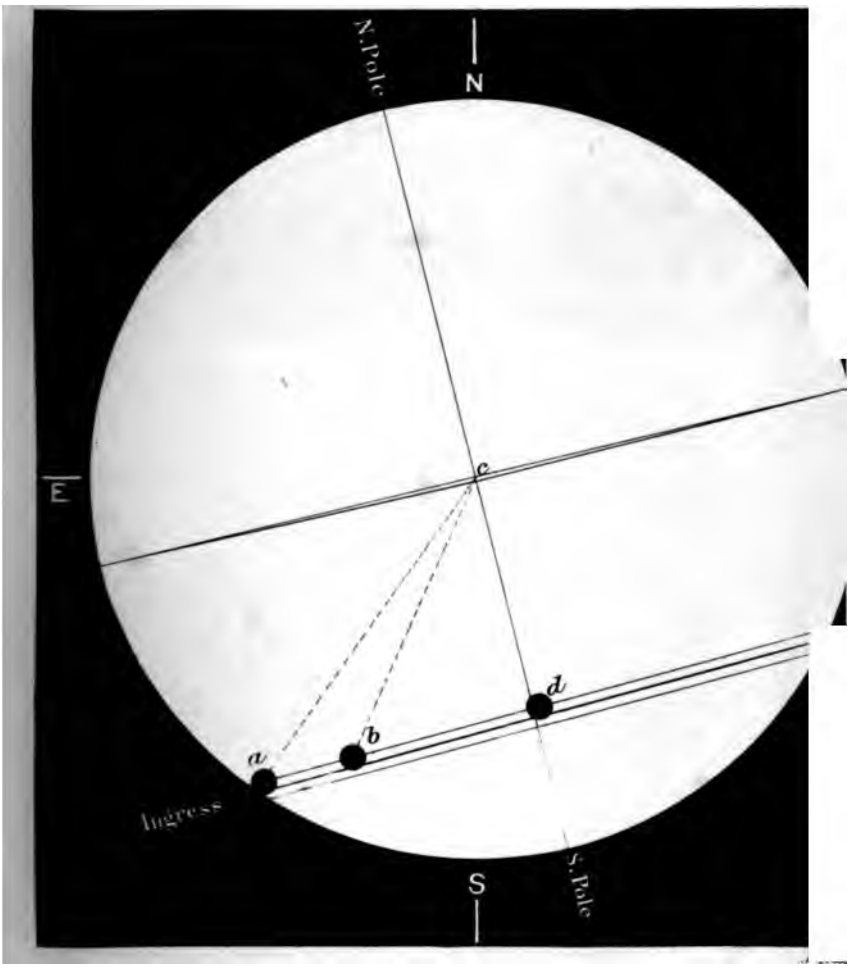


Figure 2.
1882.





July or August, 1868, 7″·94. His drawings are accompanied by a scale of 8″, from which it appears that on the 26th of August the diameter of the crater was 1″·9. With these data we obtain the following dimensions of the features observed by Tacchini, viz. the crater and a small dark spot in the middle of the white spot. This dark spot he regarded as a central pit—

Diameter of Crater	= ·35737
West shoulder	= ·09404
Floor?	= ·15047
East shoulder	= ·11285
Central pit (orifice)	= ·03762

Assuming that the surface around the cone and probably within the crater is strewed with ejected material, more or less fragmentary, resembling *broken glass*, as Mr. Huggins has suggested, we have to a certain extent an explanation of the appearance of the white spot *upon* the disappearance of the shadow. At sunrise the greater inclination of the west slope will cause it to appear as a bright, well-defined spot; the east slope being very gradual, the sun soon attains an altitude greater than the inclination of the slope, and the bright spot becomes merged in one of more than double its diameter, immediately upon the disappearance of the shadows, which is very rapid. The observation of the disappearance of the shadow after sunrise and its reappearance before sunset will assume some degree of importance, as any change in these phenomena with regard to solar altitudes will indicate corresponding changes in the surface around the orifice. On the other hand, if the surface remain in the same quiescent state the shadows will disappear and reappear with the same solar altitudes.

J. H. Mädler to W. R. Birt.

"*Bonn, October 7th*, 1868.

"Dear Sir,—The joined copy of a letter from the parts of Mr. Tacchini, received 22nd September, with the original drawing accompanying it, will be, as I believe, a precious document regarding the crater *Linné*. Therefore I have the honour to send you both of them.

"I take this occasion to remark, that the great whitish spot surrounding *Linné*, which I have found in the English observations and in the drawing of Mr. Tacchini, is never seen by me in 1831. The crater was surrounded by the greenish colour of the *Mare Serenitatis*.

"We have finished the translation of the preface of my work of the Moon, which expresses clearly my opinion about its contents. I shall send it in the next to Mr. Goodwin, to look it over on account of the language, adding the request to send it to you directly. If only the meaning is clearly understood we think to go on with the translation."

M. Tacchini to Professor Mädler.

"*Reale Osservatorio, Palermo, 16 Sept.* 1868.

"Dernièrement j'ai fait des observations sur le cratère *Linné*, et je me permets de vous en envoyer quelque détail, et même des dessins, que vous pourrez confronter avec les votres, puisque ce n'est qu'à une autorité aussi respectable que la votre qu'il peut être donné de juger sur les prétendus changements du cratère.

"Dans le nuit du 26 Juillet j'ai observé l'ombre intérieur du cratère, comme l'indique la fig. 1. Dans le 28 les conditions atmosphériques étaient encore bonnes, et dans le cratère je n'ai plus observé l'ombre aussi marquée, mais dans la partie occidentale on observait seulement une trace d'une légère pénombre, qui donnait encore l'indice d'une faible cavité, fig. 2; en profil le *Linné* serait donc comme la fig. 3, tandis que les autres cratères environnants avaient encore une ombre bien nette et noire. Dans le 29 le cratère apparaissait complètement blanc, alors j'ai porté à conclure que le cratère n'est plus dans les conditions enrégistrées par vous.

"Dans la lunaison suivante j'ai répeté mes observations, et j'ai été encore plus heureux. Dans la nuit du 24 Août le *Linné* était à peine sorti de l'ombre, voyez fig. 4. Dans le 25 le cratère était bien distincte, et le contour bien marqué à l'occident, et presque invisible dans la partie opposée. Dans le 26, avec beaucoup de surprise, j'ai vu encore la cavité de *Linné*, mais plus restreinte, et semblait une petite tache a demi-noire dans le milieu d'une tache blanche, fig. 5, mais jamais une ombre nette comme dans les autres cratères voisins, le terminator passait par *Timocharis*. Ce petit cercle sombre ne correspond donc pas à l'espace renfermé par le contour du cratère, mais il est un creux central; alors le profil de *Linné* serait comme la fig. 6. La tache blanche toute entière, c'est-à-dire avec le peute extérieure, serait selon mes mesures $7''.94$.

"Dans votre lettre au R. P. Secchi vous dites que le cratère montrait l'ombre de l'intérieur encore, quand les autres cratères environnants en étaient dejà depourvues. Donc le cratère a changé véritablement de forme, du moins dans l'intérieur.

"Eh bien, d'après cela, je dois vous adresser une prière, peut être sera trop hardie, mais j'espère d'être favorisé pour le bien de la science.

"Pourriez-vous m'envoyer une copie de vos dessins? Je les publierais en lithographie, et alors la question sera, je crois, definitivement resolue, car avec des simples mots il est toujours difficile de s'entendre.

"Agréez, Monsieur, mes salutations bien distinguées,

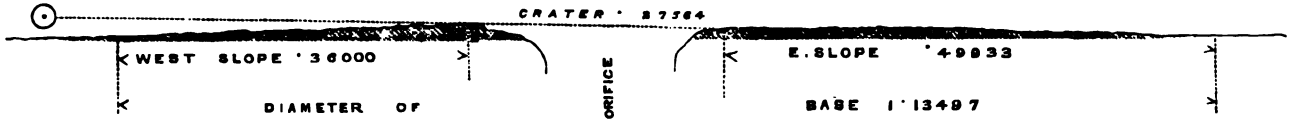
"Tacchini Pierre,

"*Astronome adjoint de l'Observatoire de Palermo.*"

Section of *Linné*, from a Drawing by Mr. Huggins, 1868, June 26.

Length of Shadow = 1·00000.

Sun's Altitude at disappearance of external Shadow.



Section of *Linné* from Drawings by Mr. Huggins and Prof. Tacchini.

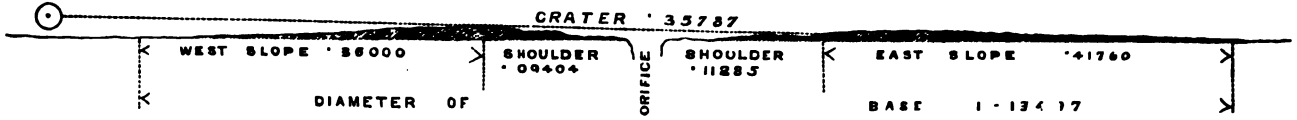
1868, June 26 and Aug. 26.

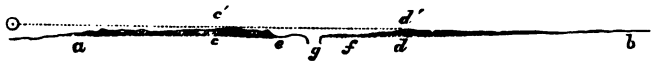
Measure of White Spots 7″·94.

Height of Cone, $\frac{14}{1000}$ of Shadow.

Sun's Altitude at disappearance of external Shadow 1° ±.

Length of Shadow = 1·00000. Sun's Alt. 0° 46′·5.





Section deduced from drawings by Messrs. Huggins, Noble and Tacchini, and a measure by Mr. Buckingham on June 26 Length of shadow = 5″·75. Sun's altitude, 1° ±.

	English miles.	English feet.
The Cone—		
Height of west rim *c c'*	..	492
Height of east rim *d d'*	..	265
Base *a b*	7·8	41282
West slope *a c*	2·4	13094
Crater *c d*	2·5	12998
East slope *d b*	2·9	15159
The Crater—		
West terrace *c' e*		3420·4
Floor *e f*		5472·9
East terrace *f d'*		4104·5
The Orifice—		
The central pit of Tacchini *g*		1368·2
Line of Section east and west nearly.		

The dotted line from ⊙ represents the line of the Sun's rays at the time when the shadow on the exterior towards the east disappears.

Although the preceding deductions are presented not only in the form of, but are actually the results of, observation, and not taken from an *imaginary* section, yet they must be received as approximations only, serving rather as a guide to observers than as being expressions of the *exact* state of *Linné* at the present epoch. It is nevertheless probable that within certain limits the features above specified will be found normally correct.

Elements of New Minor Planets. By Professor Watson.

I have determined the orbits of the new minor planets lately discovered by me, and the results are the following,—

Elements of Hecate (100).

Epoch = 1868, Sept. 1·0, Washington M.T.

$$\left.\begin{aligned} M &= 10^\circ\ 5'\ 30''\cdot4 \\ \varpi &= 304\ 45\ 0\cdot5 \\ \Omega &= 128\ 28\ 37\cdot7 \\ i &= 6\ 33\ 34\cdot6 \end{aligned}\right\}\ \text{Mean Equin. 1868·0.}$$

$$\varphi = 8\ 39\ 32\cdot5$$

$$\log a = 0\cdot493331$$

$$\log \mu = 2\cdot810010$$

$$\mu = 645''\cdot669.$$

Elements of Helena (101).

Epoch = 1868, Sept. 13·5, Washington M.T.

$$\left.\begin{aligned} M &= 17^\circ\ 48'\ 53''\cdot0 \\ \varpi &= 328\ 40\ 51\cdot0 \\ \Omega &= 343\ 35\ 0\cdot1 \\ i &= 10\ 4\ 19\cdot5 \end{aligned}\right\}\ \text{Mean Equin. 1868·0.}$$

$$\log a = 0\cdot410460$$

$$\log \mu = 2\cdot934317$$

$$\mu = 859''\cdot640.$$

Elements of Planet (102).

Epoch = 1868, Oct. 12·5, Washington M.T.

$$\left.\begin{aligned} M &= 33^\circ\ 43'\ 52''\cdot6 \\ \varpi &= 326\ 15\ 4\cdot8 \\ \Omega &= 135\ 56\ 56\cdot4 \\ i &= 5\ 21\ 35\cdot2 \end{aligned}\right\}\ \text{Mean Equin. 1868·0.}$$

$$\varphi = 4\ 37\ 37\cdot6$$

$$\log a = 0\cdot431728$$

$$\log \mu = 2\cdot902415$$

$$\mu = 798''\cdot758$$

Elements of Planet (104).

Epoch = 1868, Sept. 13·5, Washington M.T.

M =	316° 9′ 43″·2	
ϖ =	62 11 55·4	Mean Equin. 1868·0.
Ω =	43 46 42·1	
i =	2 53 26·7	
φ =	11 22 53·3	

$\log a = 0{\cdot}502401$

$\log \mu = 2{\cdot}796405$

$\mu = 625''{\cdot}756.$

Elements of Planet (105).

Epoch = 1868, Oct. 12·5, Washington M.T.

M =	106° 13′ 49″·8	
ϖ =	242 36 17·8	Mean Equin. 1868·0
Ω =	187 54 1·8	
i =	21 38 59·0	
φ =	10 8 54·0	

$\log a = 0{\cdot}376567$

$\log \mu = 2{\cdot}985156$

$\mu = 966''{\cdot}398.$

Elements of Planet (106).

Epoch = 1868, Oct. 10·5, Washington M.T.

M =	346° 44′ 32″·5	
ϖ =	35 37 53·6	Mean Equin. 1868·0.
Ω =	62 42 38·9	
i =	4 41 33·2	
φ =	11 14 46·0	

$\log a = 0{\cdot}505287$

$\log \mu = 2{\cdot}792076$

$\mu = 619''{\cdot}550.$

Ann Arbor, 1868, *Nov.* 23.



Observations of Jupiter's Satellites. By J. Joynson, Esq.

			h	m	s	
1868, Sept. 1	I.	Sat. I. first contact	10	39	55	G.M.T.
		last do.	10	45	45	
8	II.	Sat. Oc. R. noted	9	57	31	
		last contact	10	1	25	
	I.	Shad. I. noted	11	39	25	
		full on	11	43	39	
	I.	Sat. Tr. I. first contact	12	23	8	
		last do.	12	29	24	
	I.	Sat. Tr. E. last contact	14	36	17	
Oct. 10	I.	Sat. Tr. I. first contact	8	9	26	
		last do.	8	15	47	
	I.	Sat. Tr. E. first contact	10	18	13	
		last do.	10	23	28	
	I.	Shad. E. first contact	10	32	41	
		last do.	10	36	6	
11	I.	Sat. Ec. Reappearance	7	47	59·9	
17	II.	Sat. Oc. D. first contact	7	44	34	
		last do.	7	52	24	
	III.	Shad. E. first contact	7	58	30	
		last do.	8	8	10	
	I.	Sat. Tr. I. first contact	9	52	13	
		last do.	9	57	18	
	I.	Shad. I. noted	10	17	23	
		full on	10	20	45	
	II.	Sat. Ec. Reappearance	11	4	11·1	
	I.	Sat. Tr. E. partly off	12	7	18	
		Interrupted by cloud.				

Waterloo, near Liverpool.

ERRATA.

Monthly Notice for November 13, 1868.

Page 8, line 19 from bottom, *for* same contact, *read* some contact.

— 12, — 5 from bottom, *for* $22^h\ 2^m\ 35^s{\cdot}7$, *read* $21^h\ 2^m\ 35^s{\cdot}7$.

— 13, Observations by H. J. Carpenter, *for* 12^h, *read* 21^h.

— 17, line 16 from top, in reference to the phase of the egress observed by Mr. Dunkin, *for* figs. (2) and (3), *read* figs. (1) and (2).

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *January* 8, 1868. No. 3.

Admiral MANNERS, President, in the Chair.

Rev. R. H. Blair, Worcester;
E. Clodd, Esq., Edmonton;
Capt. Haig, R.E., Poonah;
Howell Jeffreys, Esq., Balliol College, Oxford;
James Knight, Esq., Glasgow;
Lt.-Gen. Lane, Sandown, Isle of Wight;
Rev. J. Saywell, Bromsgrove;
A. F. Smith, Esq., Training College, Borough Road;
E. J. White, Esq., Observatory, Melbourne; and
Lieut. Wither, Tangier Park, Basingstoke,

were balloted for and duly elected Fellows of the Society.

On the Solar Eclipse of August 18th, 1868.

By Warren De La Rue, Esq.

Under date of November 9th, 1868, Major Tennant sent to me, from Calcutta, enlarged paper copies, a little over two inches in diameter, of each of six photographs he had obtained of the eclipse; also a negative photographic enlargement of the Great Horn (which I call A), about nine times the size of the original negative; and, lastly, a photograph of a drawing of this great prominence made by Mr. James on a scale about three times the size of fig. 7.

With reference to these photographs Major Tennant makes the following remarks:—

"In each of the photographs the corner next the Great Horn (A) is very nearly where the *apparent* vertex is in an inverting telescope. The inclination of the wire (in Nos. 1 and 2) was 39° 15′ to the parallel, and the position angle of the horn is 75°."

I suggested to Major Tennant, before he left England, that it would be advantageous, in order to refer the position of the prominences to the Sun's centre, to take enlarged pictures on glass, and to etch them according to the plan I showed him, and which I had used for the eclipse of 1860, and obtained, by the electrotype process, the copper multiples from which were printed Plates xiii., xiv., xv., &c., in the *Philosophical Transactions*, 1862. In reference to this suggestion, Major Tennant says:—"The large pictures were thin and poor, and there was no use in treating them like yours by etching; for the real thing would be better shown by a distribution of the transparencies, so as to make them generally accessible."

Major Tennant has accordingly sent eight sets of transparent copies on glass of his eclipse-pictures to the Astronomer Royal for distribution; one set of these is for me, and when they come into my hands* I intend to try and procure from them etched plates like those which I made of the eclipse of 1860, as I have more leisure than Major Tennant could command, and also appliances for doing so which he does not possess. In the meantime, however, I have worked on the paper copies, and although the results I have obtained must be considered merely as first approximations, I venture to think that they may have some interest for Astronomers, inasmuch as they afford data for referring the prominences seen at different stations to a meridian drawn through the Sun's centre, thus offering facilities for a complete study of the whole phenomena observed at the various stations along the line of the central eclipse.

The only prominence visible *throughout* the totality at Major Tennant's station at Guntoor was the Great Horn (position-angle 75° in reference to the Moon's centre). Prominence G first became visible at the epoch of No. 5 photograph; at the epoch of No. 4 only the Great Horn, A, was depicted. Prominence C is completely covered in No. 3, and prominence B partially so, as well as the soft light indicated by dotted lines in the diagram, close to B towards the north, position angle 116° to 125°, height above the Moon's limb 1′ 41″. The outline of this soft light is not changed in the interval between 2 and 3, and it cannot, on account of its well-defined form, have arisen from light reflected from the photosphere by the Sun's atmosphere. Another patch of light, with a softened but characteristic branching outline, is visible between the prominences B and C, position angle 133° to

* I received these photographs from the Royal Observatory on Jan. 22d, while this paper was passing through the press. They contain much more detail than is visible in the paper copies; for example, in No. 1, a band of low prominences can be seen in the midst of the glare, hereinafter alluded to, extending from position angle 96° right up to prominence B. In No. 2 is a very curious group of faint prominences K making up half an ellipse, from position angle 65° to 72°, the minor axis of the ellipse extending about 40″ above the Moon's limb. These pictures are extremely interesting, and, in my opinion, must be considered as eminently successful results.—Jan. 23, 1869. W. D. L. R.

139°, height above the Moon's limb 2′ 27″. Similar appearances are indicated in my eclipse-pictures of 1860, and form part of the appendages of the Sun. These entities, bounded by outlines which blend almost imperceptibly into the general light of the corona, are deserving of especial study, which will be best accomplished by means of photography, as it truly records their faint contours, which are likely to be overlooked in eye-observations, because they are lost in the softened light which surrounds the Moon; and also because the eye is naturally attracted most by the prominences which have a distinct outline.

The wire of which Major Tennant speaks in Photographs Nos. 1 and 2 can be well discerned in the first, but is seen with difficulty in the second picture. In none of the paper copies can the Moon's limb be traced beyond a little more than the half of its circumference;* and, at first, it did not appear possible to discuss these pictures graphically in the same way as I had done my own photographs of 1860, but ultimately I was enabled to make out the diagram which I now lay before the Society; the work was accomplished in the following manner:—

By very careful measurements the centre of each picture was first found, and a circle representing the lunar disc then described with the dry point of the compasses.

Taking the angle of position of the Great Horn (which did not change greatly during the totality†) as 75° in all the pictures, the line N S was ascertained and also drawn with a dry point. A careful tracing was then made of the pictures 1, 2, and 6, and the tracings were in such a manner superposed that the Great Horn coincided in all, and that the lines N S, N′ S′, N″ S″, were seen to be as nearly as possible parallel. By these means the relative positions of the Moon's centre at the epochs of No. 1 and 6 were obtained. By drawing the circle to represent the Sun in such a position that the lunar peripheries at epochs Nos. 1 and 6 were tangential to it, the radius of the Sun was ascertained and the position of his centre at the same time laid down. It was then possible to obtain the following elements, as may be seen by an inspection of the diagram, which is a reduction, to about the same size as my original photographs of 1860, of a larger diagram, drawn out on a scale of half an inch to a minute of arc:—

Orbital motion of the Moon's centre during the totality, referred to the Sun, was a parallel to a diameter drawn from	285° to 105°
Orbital angle therefore	15°

* In the glass copies the Moon's limb can be traced nearly all round in photograph 5, and completely so in 6.

† This angle changed about 2° 50′ in reference to the two positions of the Moon's centre in photographs Nos. 1 and 6; but this was only ascertained as the diagram was developed; for this reason the lines N S and N″ S″ could not be made quite parallel when tracings of the photographs were superposed.

Measured orbital motion during the interval between Nos. 1 and 6, namely $5^m\ 31^s{\cdot}9$, reduced to seconds of arc*	132″
The relative motion of the Moon's centre in Right Ascension during the interval between Nos. 1 and 6	W. to E. 128″
The relative motion of the Moon's centre in Declination during the interval between Nos 1 and 6	S. 32″·4
[The relative motion *during the totality*, namely in in $5^m\ 45^s$, would consequently have been:— in R.A. 133″·5 and in Decl. 33″·7.]	
Position-angle of 1st internal contact, or point where the light disappeared	101°
Position-angle of 2nd internal contact, or point where the light reappeared	294°
Nearest approach of centres	7″
Ratio of the Moon's semidiameter to the Sun's semidiameter = 1	1·07188

Prominence.	Position Angle referred to Sun's Centre.		Height above Sun's Limb.	Height above Moon's Limb.
	°	°	′ ″	′ ″
A	76·0 to	81·5	3 10	3 4†
B	123·0	127·0	1 0	0 54
C	139·0	148·5	1 32	1 19
D	262·0	263·0	0 22	0 13
E	283·9	287·5	0 54	0 54
F	295·5	298·5	0 44	0 44
G	314·5	322·0	1 7	1 0
H	326·0	331·0	0 45	0 32
I	335·5	337·5	0 48	0 27
K	65·0	72·0	...	0 40‡

In the diagram I have inserted the position of the Sun's spots and faculæ, as photographed at the Kew Observatory.§

Sun-spots do not appear to have had in August, 1868, more than in July, 1860, any immediate connexion with the prominences. A patch of faculæ, however, between position-angles 138° and 147°, extending up to the limb of the Sun, corresponds well with the position of prominence C. And the patch of faculæ 255° to 257° nearly so with the small prominence D.

Indeed, so far from anticipating that there could be any prominences surmounting Sun-spots or in immediate contiguity with them, the published researches in Solar Physics by myself, Mr.

* The value of the arbitrary scale employed was reduced to seconds of arc by taking the Sun's semidiameter to be 15′ 50″·6, and deducing therefrom the value of 1″ on the scale.

† These heights are exclusive of the curved point *a*, fig. 7, which cannot be seen in the paper photographs; its length is about 12″, making the total height of the Great Horn 3′ 22″ above the Sun's limb.

‡ Not shown in the diagram, as it is not visible in the paper copies.

§ *Monthly Notices*, vol. xxix. p. 3.

Balfour Stewart, and Mr. Loewy, induce us to think that there ought to be an absence of these appendages where Sun-spots exist. The prominences are evidently more closely connected with the faculæ, for we now know with certainty that they arise from an uprush of heated gases, whereas we have stated that Sun-spots are closely connected with a downrush of comparatively cold gaseous matter.

The positions of the Sun's axis, equator, and two parallels of latitude, 30° N. and 30° S. in the diagram, are derived from the following data:—

	° ′
Inclination of the Sun's Axis to the East of the Meridian through the Sun's centre	17 24·9
Elevation of Earth above Sun's Equator	+6 52·6

Major Tennant, in a previous letter, communicated to me his observations of the spiral structure of the Great Horn, A, which was printed in the *Athenæum*, October 3, 1868, No. 2136, with some remarks of my own, in which I gave the credit to Major Tennant for having first pointed out this peculiarity of structure; but, at the same time, I called attention to the fact that a similar spiral structure was visible in my photographs of the Eclipse of 1860, for example in the prominences C and D, as depicted in my plates Nos. ix. to xv. *Phil. Trans.* 1862. In his letter of November, 1868, Major Tennant adds the following statement:—"About the Horn: I trust there can be no doubt about its structure, but I would draw attention to the closeness of the spiral low down, when first formed, and its size above, which seem to me to accord with my theory. Indeed, I almost feel that there are two close spirals in the lower part of the luminous vapour, each partaking of the general motion, and in the upper part coalescing. There should be, according to Dr. Vogel's account, a sensible change in the 5½ minutes, and I think I have mentioned that I suspected one before I saw his paper. It is quite clear that, from some cause, the latter photographs are taller than the first; what puzzles one is the white hole (Fig. 7 *b*). In No. 1 one sees the knob outside it; and in No. 6 a little of it is seen in the same place; as it is in all between these two pictures. At the time this seemed to me conclusive against a change, but I am disposed now to believe that this could be explained otherwise, and that the change of height is real, and not an accident of exposure entirely. My theory would require the change, but that was not formed until recently, long after my suspicion had been given up as too uncertain for evidence, and I did not hear of Dr. Vogel's result until after my paper was ready for dispatch."

The paper copies are not sufficiently distinct to enable me to ascertain whether there was any change in the aspect of the Great Horn during Major Tennant's observations,* but it is very

* From a cursory examination of the glass copies, I believe that they contain evidence of an axial rotation of Prominence A.—Jan. 23, 1869. W. D. L. R.

probable that there was, for there is reason to suspect that this prominence was undergoing rotation; and I call attention in support of this view to the very different appearances of prominence A depicted by various observers along the line of totality.

I am dependent in the main on the representations of the phenomena given in the *Engineer*, of November 6 and November 13, for the appearances presented by the Great Horn (A) at Aden, on board the ships Rangoon, lat. N. 15° 42′, long. 59° 15′ E., at Beejapoor, at Wha Tonne in the Malayan Peninsula, and at Labuan. My friend, Mr. W. Fane De Salis, one of the Directors of the Peninsular and Oriental Company, kindly sent me, in September, 1868, a tracing of Captain D. Rennoldson's drawings, and I find that two of them are faithfully represented in figs. 14 and 15 of the *Engineer*, and this fact inspires confidence in the other pictures published in that journal. Fig. 2 is a copy of one of the tracings sent to me.

On reference to the subjoined diagrams, all drawn out to the same scale as that representing Major Tennant's photographs, it will be seen that in the Aden photograph, obtained on Aug. 17, $15^h\,30^m\,39^s$, the Great Horn was so curved that its point was directed northward, see fig. 1; in fig. 2, representing the appearance observed on board the steam-ship Rangoon, the direction is even more decidedly northward; this observation occurred at $15^h\,39^m\,18^s$, or nine minutes after that at Aden. In fig. 3, as observed at Beejapoor at $16^h\,1^m\,32^s$, after a further interval of 22 minutes, the point is directed southward. At Guntoor, the extremity of the Great Horn still pointed southward, but not so markedly; the appearance of it is represented in fig. 4, and occurred at $16^h\,10^m\,52^s$. So that it is probable that the Great Horn performed half a rotation on its axis between the observations at Aden and those at Guntoor, that is, in 40 minutes. Fig. 5 shows the appearance of the Great Horn as observed on the Malayan Peninsula, at about 17^h; in this case the extremity points northward, as at Aden, and it appears to have performed half a rotation in the interval between the Guntoor and Wha Tonne records, that is, in 49 minutes; and at Labuan, at $17^h\,52^m$, the direction of the extremity is southward, and the prominence had possibly performed another half a rotation in 52 minutes, the velocity having progressively decreased. Between Aden and Labuan one-and-a-half rotation was apparently accomplished in a direction from left to right, looking from the Sun's centre towards the prominence.*

I am not aware whether the representation of the Aden observation in the *Engineer* was copied from an actual photograph or from a print which had appeared in another publication. The hypothesis which I indicate of the probable rotation of the Great Horn depends, however, very much upon the faithfulness of the woodcut in the *Engineer*, and until prints of the Aden photo-

* The direction of the rotation is inferred from that of the convolutions of the spiral of the Great Horn.

graphs are compared with those of Guntoor it will not be possible to speak with certainty on the point, nevertheless I have thought it desirable to draw attention to the subject.

In Major Tennant's photographs, and in my own of 1860, the deep indentation of the concave side of the prominences is very striking, leading to the impression that they do not actually touch the photosphere, but are elevated at some little distance above it.* The interval must be extremely small, for in 1860 I did not notice any luminous ring between the Sun's limb and the prominences, nor was any depicted in the photographs; but at the recent eclipse there appears to have been remarked by more than one observer that there was an indication of such a brilliant ring of light. In Major Tennant's first photograph, between position-angle 80° and 120°, is a large halo (covering an area equal to about $\frac{1}{4}$ of the lunar disk), which prevented part of the surroundings of the Sun's limb from being clearly depicted, produced evidently by a strong light, and in reference to this he makes the following remarks in his letter of November 9, 1868:—

"I wish to call your attention to the bright edge of light in No. 1, which I believe to be the stratum of heavy luminous vapour. It may be said that there is a glare, and this may be the residual sun; but the cusps extend beyond the glare, and it would have been hidden only after 10 seconds, while Phillips says (what is confirmed by other testimony) that the Sun was lost very shortly indeed after the exposure. I am quite satisfied this was not the solar disk, and if there be any question about it, I hope you will be able to confirm my opinion."†

Under date of November 28, 1868, Major Tennant, in relating a conversation he had had with M. Janssen, says, "His account of the passage of the Moon's disk over the edge of the Sun puzzles me. As he puts it, the bright surface in my No. 1 must have been the Sun itself, but if so, the limb must have been very pale beyond all I ever saw. He, however, said he could not be certain that a narrow line did not exist, when I pressed him."

I cannot from the examination of the paper copies say with certainty whether the bright glare was or was not due to the residual Sun, but I rather inclined at first to the belief that it was not due to the Sun.† This opinion was founded on my experience of 1860, when a very minute crescent of the Sun was taken, in No. 28 photograph, when the versed sine of the Sun's cusp was only 9″·6, by an extremely rapid uncovering and recovering of the object-glass; the instantaneous slide in the Kew heliograph not having been brought again into action for the partial phases after it had been removed for the totality pictures, until the epoch of the next succeeding photograph. Although, in the case of the Kew heliograph, the image, in consequence of its enlargement by

* In the glass Photograph 6 are depicted several perfectly detached prominences which are not seen in the paper copies.

† A careful examination of glass photograph No. 1 has convinced me that the glare was not due to a cusp of the Sun.—Jan. 23, 1869. W. D. L. R.

the secondary lens, was reduced in intensity 64 times, yet the plate was so solarised that no trace of prominence A, which was visible at the time, was depicted; whereas in No. 1 of Major Tennant's photographs very minute bead-like prominences near the Great Horn, together with this and the prominences B and C, are clearly recorded.*

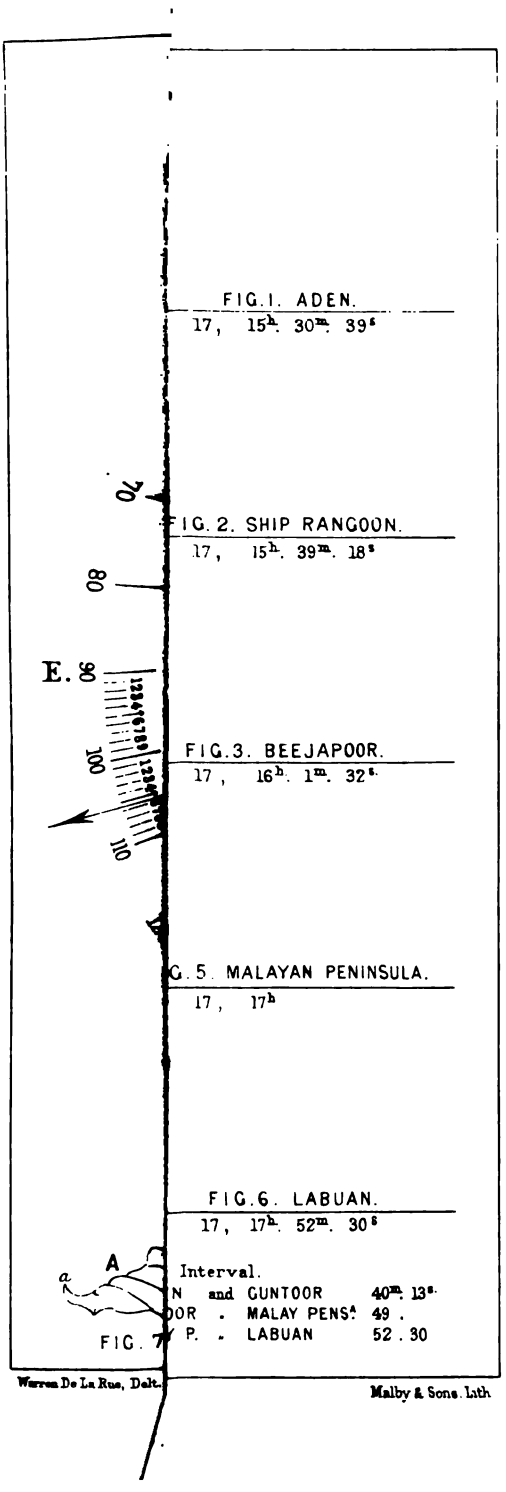
The position angle (namely 111°·5) of the centre of the glare is 10°·5 to the South of the first internal contact, and this halo could not have been caused by the last traces of a cusp of the Sun, which would have been to the *North* of the first internal contact, but a furrow on the Moon's limb may have allowed the passage of a minute beam of the Sun's light, and this would have been quite sufficient to have produced the glare, and I think this is a probable explanation of the appearance, but I reserve for a future communication my final opinion on this point.

In the account given by Capt. Haig, R.E., in the *Proceedings of the Royal Society*, No. 105, 1868, of his observations near Beejapoor, he gives the following extract from the notes of Capt. Tanner, who took part in them:—"As regards the corona, when we first began to see the Eclipse through the clouds, I was under the impression that the eclipse, instead of being total, was only annular, so bright was the corona near the Moon's limb. I could not detect any irregularities in the light of the corona, but the light appeared to be gradually shaded off all round." In this bright light are evidently two distinct phenomena mixed up together, namely, the brilliant self-luminous entities with softened outlines, before spoken of, and the true corona caused by the reflection of the Sun's light by his non-luminous atmosphere.

I quote from the *Engineer*, November 13, 1868, page 371, with respect to the observations of M. Stéphan on the Malayan Peninsula: "After the side of the Sun had disappeared the Moon was seen by M. Stéphan and M. Tisserand as bordered by a thick luminous ring for about a quarter of a minute; this ring was brighter than the surrounding sky, and was sufficiently brilliant to cause an error as to the exact moment of the contact. It reappeared a few seconds after (before?) the third contact."

Although the extent of the ring of light is evidently estimated at far too large a quantity for it to be situated beneath the underside of the prominences or to have escaped depiction in the photographs, and is, in all probability, due to the same causes as the phenomenon described by Capt. Tanner; yet it would appear that there are still some doubts left to be settled, and that, notwithstanding the means we now possess of studying the Sun's appendages when he is not eclipsed, we must wait for future total eclipses before we can ascertain with certainty whether there

* On the occasion of the reading of this paper, Serjeant Phillips was present, and he stated that in his opinion the glare might have been caused by residuary sunlight (for the signal to expose the plate was given to him before the Sun completely disappeared), and that he purposely delayed doing so for few seconds, until he judged by the *unassisted eye* that the Sun had become covered.


FIG.1. ADEN.
17, 15h. 30m. 39s
FIG.2. SHIP RANGOON.
17, 15h. 39m. 18s
FIG.3. BEEJAPOOR.
17, 16h. 1m. 32s.
G.5. MALAYAN PENINSULA.
17, 17h
FIG.6. LABUAN.
17, 17h. 52m. 30s
Interval.
N and GUNTOOR 40m. 13s.
OOR . MALAY PENSA 49 .
P. . LABUAN 52 . 30
FIG. 7
A
a
70
80
90
100
110
E.
Warren De La Rue, Delt.
Malby & Sons. Lith.

is any self-luminous atmosphere between the photosphere and the underside of the prominences.

It will, on future occasions, be extremely desirable to have photographic observations made as nearly as possible on both the Northern and Southern limits of the shadow as well as at stations situated near its centre.

Moreover, it would be very desirable to have two precisely similar instruments at the same station, in one of which the sensitive plates would be exposed for 1 second, and in the other instrument from 30 seconds to 1 minute, in order to obtain data for the correct interpretation of all the phenomena.

In connexion with the observations of the Sun's appendages when it is not eclipsed, it may be permitted to recall to recollection my estimates of the relative brightness of the prominences and the solar disk,* namely, that the former were in 1860 from 696 to 1111 times less brilliant than the Sun; I think this will be confirmed when the relative dispersion of the Sun's light and that of the prominences by the spectroscope is calculated; this of course depends on the dispersive power of the prisms and the opening of the slit. If, for example, the solar spectrum is dispersed over a space of 10° or 600′, and the bright lines occupy together about 2′, the light of the prominences would only be three times less brilliant (according to the mean of the estimates I have quoted above) than the Sun, and might in consequence be seen on the spectrum of the disk, which we know is possible; it would of course be much more brilliant than that of the spectrum of our atmosphere in contiguity with the Sun.

Mr. Hind has, since this paper was written, been so good as to give me the following data, calculated for the assumed position of Major Tennant's station:—

	Guntoor.
Longitude	80° 27′ East.†
Latitude	16 15 North.
Middle of Eclipse, 1868, Aug. 17,	h m s 16 10 52 M.T. at Greenwich.
Duration of totality ..	0 5 45
Sun's semidiameter ..	15′ 50″·6
Moon's apparent or augmented semidiameter	16 59·0
Position-angle of first internal contact, or point where light first disappeared	103° N. towards E. direct image.
Position-angle of last internal contact, or point where light reappeared	71° N. towards W. direct image.
Nearest approach of centres	3″·9
Moon's apparent hourly motion in R.A.	23′ 27″·3 E.
,, ,, Decl.	6′ 32″·1 S.

* From two different sets of considerations, *Phil. Trans.* 1862, p. 405.

† The position given in Major Tennant's paper is—

Long. E. 80° 27′ 9″; Lat. N. 16° 17′ 29″.

[Jan. 29, 1869. W.D.L.R.]

Moon's apparent Decl.	13° 3′ N.
Orbital angle is therefore	15° 58′

A comparison of the data deduced from Mr. Hind's numbers and my own is subjoined:—

	Hind.	De La Rue.
Position angle of first internal contact, N. E. S.W.	103°	101°
Position angle of second internal contact ..	289°	294°
Nearest approach of centres	3″·9	7″
Moon's apparent motion during the totality in Right Ascension E.	134″·86	133″·5
Moon's apparent motion during the totality in Declination S	37″·59	33″·7
Ratio of semidiameter of Moon to semidiameter of Sun = 1	1·07195	1·07188
Orbital angle	15° 58′	15°

The data obtained graphically will be found to be not very discordant from those calculated, especially when it is taken into consideration that they were obtained from paper photographs on so small a scale as those at my command.

The Society has received from Major Tennant a detailed Report* of his Observations of the Solar Eclipse of 18 August, 1868, as seen at Guntoor, with illustrations, including the photographs and drawings referred to in the foregoing paper by Mr De La Rue: also the copy of a Report addressed to Sir Alex. Grant, Bart., Director of Public Instruction, Poona, by Mr. Kero Laxuman Chatrey, Prof. of Mathematics and Natural Philosophy at the Deccan College, of the Eclipse, as observed by him in company with Captain C. T. Haig, R.E., and Captain Tanner, at Beejapoor; the account by Captain Haig of these observations has been published in the *Proceedings of the Royal Society*. The Society has also received a communication from Col. T. Addison as to the observations of the Eclipse at Aden.

The Great Nebula round η Argûs. By Lieut. J. Herschel and Sir J. W. F. Herschel, Bart.

(*Observations of the Nebula by Lieut. Herschel, and Remarks by Sir J. Herschel, in reference to the supposed Variation of the Nebula.*)

Lieut. Herschel, in a letter to Sir J. Herschel, dated Bangalore, November 23, 1868, writes:—

* Jan. 29, 1869. I have this day had an opportunity of reading this most valuable and interesting paper, and am glad to say that my interpretation of the appearances depicted in the photographs agrees in many particulars with that of Major Tennant.—W. D. L. R.

I have this morning had a look at the Nebula round η *Argûs*. Had I known I could see it so early I should have tried sooner. It is now on the meridian (and therefore has an altitude of 20°) at sunrise, and I can see it readily two or three hours earlier.

You will understand that I do not mean to content myself with one morning's examination, and that if I send you a hasty account, it is, in some measure, to record *first impressions* purposely.

The Nebula is *easily* seen with the naked eye. The eye *selects* the object as naturally as it would choose the Pleiades at that altitude. I have not compared it *directly* with Nubecula Major (preceding by six hours), but as I happened to have looked at the latter two nights before, I may trust my memory to say that the Nebula η *Argûs* is undoubtedly intrinsically brighter.

In a binocular it appears as two elliptic patches.



The figure is from memory.

I examined it telescopically with powers of 55, 85, 125, 200, sketching the appearance in each case. These sketches I have now before me. You know how difficult it is to represent faithfully one's impression of a nebula by a hurried pencil drawing, and will understand that the accompanying *copy** (based on all) has no pretension to accuracy. *In fact it is a wretched attempt.*

There is a copy (?) of your drawing in *Chambers's Astronomy*. This I had by me; and compared with my first and second sketches after they were made, not to correct my impression or sketch, but to fix the former and to note the agreements and disagreements in the latter. I did not look at your figure (in the *Outlines of Astronomy*) until I came in.

On the whole, sketch for sketch, I see no more marked differences than I should see in a similar comparison of a sketch of the Nebula *Orionis* with your figure (on the same page).

I marked the positions of twenty-nine stars in the central part only, of 9 to 12 or 14 mags. Of these I recognized the positions of seventeen at least, as practically identical with those in your figure, four are smaller than the run of those you show, three are so close to η that the brilliancy of that star no doubt obscured them. The remaining five are unidentified and unrepresented. η itself I estimate (ignorantly) at 7 to 8 mag.

I *do* recognize differences of configuration, no doubt, chiefly in the bridging of the dark space, and partial filling up of one portion; also the presence of another space scarcely more than indicated in your figure.

Upwards of fifty Nebulæ of different degrees of brightness

* Afterwards referred to by Sir J. Herschel as the sketch marked A: see Plate A.— Ed.

have been under inspection, during the last few nights, and I think I may say that they have to some extent qualified my eye to judge of shape and relative brightness; and lastly to state that, so far as an hour and a half's study of this particular one is concerned, I do not recognize such a total "subversion of all the greatest and most striking features &c.," as Mr. Abbott's diagrams and statements appear to imply.

No doubt the relative brightness of some of the stars has altered; but, so far as I have compared, not remarkably (except η of course).

I propose to make a more precise survey of both stars and Nebula at once.

Sir J. Herschel, in a letter to Admiral Manners, dated Collingwood, Dec. 22, 1868, communicating the foregoing, writes:—

The extreme interest created by the account communicated by Mr. Abbott of the changes which would seem to have taken place in the great Nebula about η *Argûs* leads me to conclude that it would be wrong to delay communicating to the Royal Astronomical Society a letter which I have just received from my son, Lieut. J. Herschel, whom I requested to examine it so soon as its time of meridian passage would allow, with a view to ascertain, so far as the instrumental means at his command would admit, the nature and extent of the alleged changes, and at the same time to note the configuration of the stars surrounding the principal one (η). The letter is accompanied by two sketches—eye-drafts—the one (that marked A) being the one referred to more especially in his letter; the other (marked B) is not therein expressly referred to, nor indeed have I any positive ground for certainty that it is a sketch of that Nebula at all, further than that he speaks of having made sketches with each of the powers used, of one of which, probably that with a low power, I suppose it to be a copy; though it presents so little resemblance to the other as to allow of a very reasonable supposition (in the absence of any such guiding circumstance) that it had been taken for some other nebula. Indeed I should not have inclosed it at all, but as an instance of the very different impression one and the same nebula may make when viewed with different optical powers.

As the telescope with which the sketch (A) was made inverts differently from the 20-feet reflector with which my drawings were made, I annex a semi-reversed copy of (A) [marked (C)],* formed by pricking through the stars of (A), and then reversing the paper, and copying in with as much precision as possible the nebulous surroundings (semi-reversed) on the back of the paper. I have also added allineations pointing to the stars unmistakably identified, twenty in number, annexing the letters and numbers

* The semi-reversed copy (C) is not here reproduced; but see the plate (A A), which exhibits a like copy of Lieut. Herschel's second sketch of the Nebula.—Ed.

which stand attached to them in my Catalogue of the Stars of the Nebula. Of these I ought to mention that two, viz., (α) No. 736 and (γ) No. 741, whose magnitudes are rightly stated at 9 and 11 in the Catalogue, are represented much too small in the engraving, though in their right places.

Unidentified are the four stars which form a pentagon with H towards η. There are eight small stars in that area in my chart, and these are doubtless four of them not laid down with sufficient precision for the purpose of identification.

The unidentified star marked ? ? δ is a very important one. If it be really that star (a star 10 mag.) the evidence of change in the Nebula would seem to be really conclusive, since that star, when I delineated the Nebula, was carefully laid down as a landmark for future reference, as being just within (by a distinct though minute interval) the sharply-defined southern border of the Lemniscate, while three other exceedingly minute stars, with a similar view to future reference, on the south preceding border, were *just without* or *just on* the edge. But the allineations of the hypothetical δ disagree very markedly with those of the real δ, which lies in an exact straight line between C and (ζ), as also between D and G. The former condition is pretty well satisfied, but the latter is very wide of the mark.

On the whole, allowing for difference of optical power, there is no difficulty in recognizing the Lemniscate form, and my son's drawing, at all events, is decidedly adverse to the utter breaking up of its well-defined southern border by two clear open channels, which forms so marked a feature in Mr. Abbott's delineation.

Further and more elaborate delineations will undoubtedly follow.

P.S. On further consideration I am disposed to consider the sketch (B) as an attempt to delineate the clustering stars intermixed with nebula in the south preceding branch of the Great Nebula.

Lieut. Herschel's further observations, dated Bangalore, 29 Nov. 1868, are as follows:—

I examined this Nebula on the mornings of the 22nd and 23rd November with the Royal Society's 5-inch refractor, using different powers from 55 to 200, and sketching the appearance presented in each case. The second morning was devoted to one careful survey of the stellar points in and about the central portion, and to one sketch, based on it, of the nebulous light. The accompanying sketches show the appearances under powers of 55 and 125, both being drawn *afterwards* from the sketches made on the spot (the originals being retained, as I may be enabled hereafter to secure further observations). I believe they are both very faithful, but the appearances are so different, not only under different magnifying powers, but also under a more or less sustained gaze, that it is difficult to avoid giving features too marked a character,

on the one hand, and to indicate them correctly on the other. The relative intensity of the broader features is no doubt best seen under a low power, but the fact that in that case marked outlines are not traceable, which come out stronger under a high power, seems to indicate a tendency to exaggerate the latter effect. I am inclined to attribute somewhat of the differences *in this respect* between the present sketch and the figure of this Nebula in the "*Outlines*" to an endeavour on my part to avoid the difficulty by giving an intentional indistinctness to some parts about which I could not feel any confidence. It is hardly necessary to say that I carefully avoided comparing the above until I had completed my own.

Out of about sixty stars plotted, I believe not more than five or six are erroneously placed (to the extent of affecting appretiably the configuration). Of the rest by far the greater portion are easily identified with stars of similar magnitudes in the older drawing; a few are strangers, and perhaps two or three of these are (now) of sufficient magnitude to attract attention as not represented in the published diagram. In one or two cases also where there are *pairs*, their direction is more or less different; but this is hardly more than might be expected in thirty years.

The nebulous area, of which the accompanying low-power sketch forms the principal portion, is visible to the naked eye as a nucleus in the milky way. The latter appeared to me, on the morning of the 24th November, as of more intrinsic brightness than *Nubecula Major*, height for height; and of course therefore there was no comparison between the latter and the Nebula. It is an object which the eye selects at once for closer examination. Under a low power (of 3 or 4) it looks like two oval patches with many stellar points. Of these only one is shown in my sketch (2), being the portion which was naturally examined first.

There does not appear to be any very remarkable change in the distribution either of the stars or the Nebula, so far as that part immediately round η is concerned; but, if that portion formerly represented all that could be recognized in a moderate telescope, a great change must be acknowledged, arising, as it would seem, from the increased relative intensity of surrounding parts, rather than from subversion in the central.

It seems only necessary to add that the altitude at the times of observation was about 15°, and that the air was clear enough to have shown stars of the third magnitude within a degree of the horizon, roughly speaking.

Sir J. Herschel's letter, dated Collingwood, 2 Jan. 1869, communicating the foregoing, is as follows:—

Herewith I have the honour to enclose two drawings which I have received from my son, Lieut. J. Herschel, being those, I presume, alluded to in his first brief communication as intended

to be executed with greater care; as also a description accompanying them, which, if not too late for communication to the Royal Astronomical Society at their next meeting, might very advantageously be considered simultaneously with the former drawings and their accompanying comments.

I accompany my son's drawings with two others, which will serve the purpose of elucidation. The first, marked (A A), is formed by pricking off the stars laid down in his figure No. 2, of November 24, through the existing pinholes in this figure; and shows the direction of the meridian and orientation in that figure as it must have been at the time of making the sketch. It exhibits also the letters and numbers in my Catalogue of the stars in and near the Nebula with which those in his drawing are identified. Of all the stars so pricked off, forty-nine in number,* there is only one of whose identity there can be any doubt, viz., a minute star indicated in both our figures by a query ? attached. It is either the star 14th magnitude, marked as *r''*, No. 792, in my Catalogue considerably misplaced, or the star No. 650, of 17th magnitude, with which its place agrees. If his (?) is intended to express a doubt as to the actual existence of a *suspected* star in that place, the latter supposition must be adopted.

The other accompanying chart, marked (B B), is formed by pricking off all the stars in my large engraving within the compass of the paper, and then, at the back of the paper so pricked through, lettering and numbering in ink the identified stars, and those only. The semi-reversion thus effected brings the stars of this chart (when placed with the pencil line drawn through *η Argûs* and O parallel to a line through the same stars in Lieut. Herschel's figure) into correspondence as to right and left, top and bottom, with the rest of the stars in that figure, and their agreement (allowing for the difference of scales), will at once be evident.

On this chart I have also drawn in pencil the leading lines of the Nebula as it existed in 1834–38, from which I think it will be very evident (allowing for the great difference in the illuminating powers of the instruments employed) that the Lemniscate *still exists* and holds as nearly as might reasonably be expected under the circumstances the same relative position among the stars, and more especially as relative to *η Argûs* itself. In this drawing of my son's it should be noted that the *true* (δ) makes its appearance quite unmistakably, and it stands just where it ought, a little beyond the border of the Lemniscate *within the Nebula*. It is true that the outline of the Lemniscate is shown much less sharply defined, and, holding the drawing at a distance and in a good light, it may be clearly traced. The star (η), too, I may observe intermediate between (5) and (6), has by some inadvertence been

* In his description he speaks of "about 60" originally "plotted." I can find, however, only 50 in this figure, one of which accidentally lies beyond the border of the paper in my projection.

omitted, but is unequivocally shown in its proper place in the drawing previously sent.

A decisive proof of the absence of any material shifting of position between the Lemniscate and η *Argûs* is afforded by the small star marked [*v*] in a direction pointing from η to about half-way between C and D. This star (No. 664 of my Catalogue) occupies, both in my engraving and in my son's drawing, precisely the same strongly-marked position close to the very edge of the most characteristic projection of the Nebula, being *just immersed* in the Nebula in both.

The drawing No 1, with a low magnifying power, it should be observed, is on a scale only about one-third that of my engraving, and of course is intended to take in not the Lemniscate region only but the great diffused outlying masses. These, as will create no surprise to those accustomed to the difference of appearance in Nebulæ under great differences of illuminating power, offer hardly any recognizable similarity to the curious and complex detail exhibited in my engraving. Yet the Lemniscate, as a small *closed* form to the left of the small star (η *Argûs*, the middle one between the three large stars laid down), is perfectly recognizable, though much more feebly marked than I should have expected. Of these three stars I presume the two upper ones in the figure to be W and Y of my chart. That in the lower portion is beyond its limits.

The plates belonging to the foregoing paper are —

1. Sketch A, in Lieut. Herschel's first communication.
2 and 3. The drawings 1 and 2, in Lieut. Herschel's second communication.
4 and 5. The tracings A A and B B, referred to in Sir J. Herschel's second letter.

Description of an Automatic Transit Instrument.

By Sidney B. Kincaid, Esq. (Abstract.)

It is stated that the object of the paper is to describe an apparatus for the adjusting of isolated timepieces, so simple in its construction as neither to be very expensive to provide at first, nor easily liable to derangement when fixed, and so simple in its mode of use, that it might be mastered in the course of an hour by any person of moderate intellectual powers.

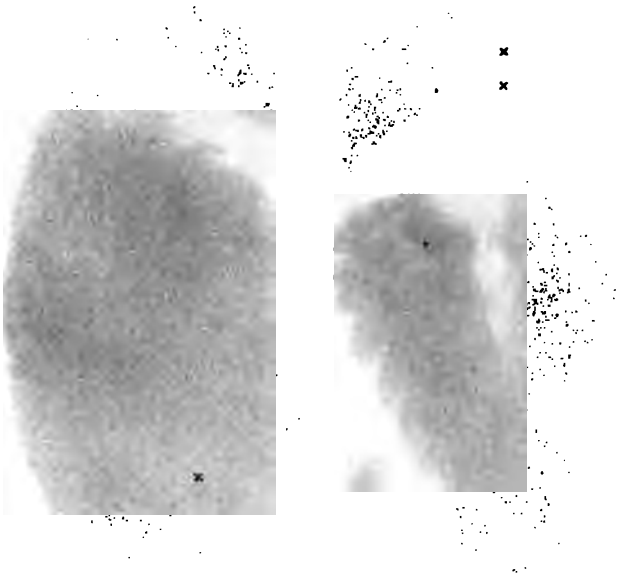
The apparatus consists of a plane mirror and a burning-glass to be adjusted in such manner that, at the instant the Sun reaches the meridian of the locality, the rays ignite a thread which burns

The Nebula round η Argûs, I.

(A)



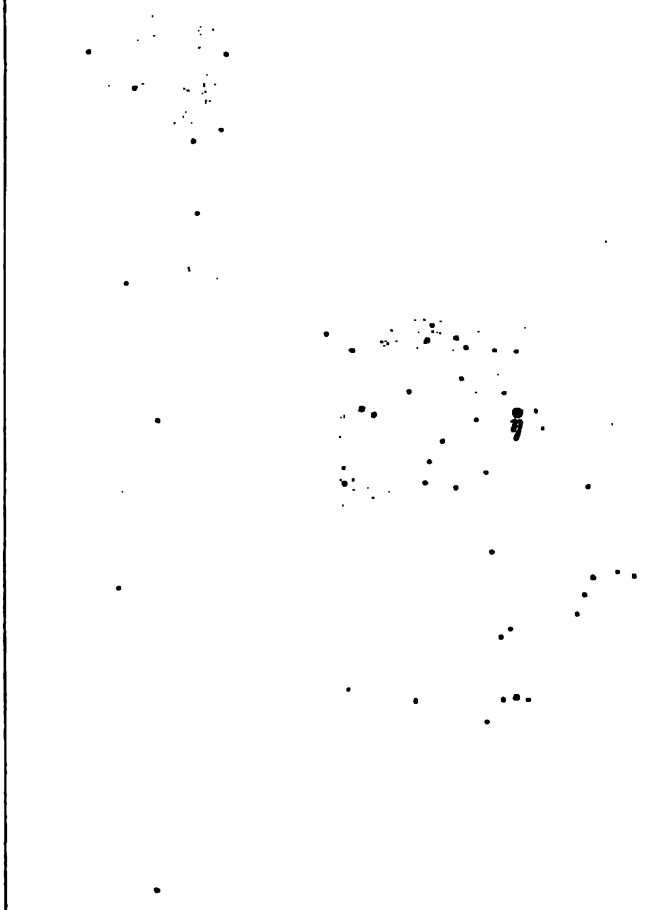
Malby & Sons



Approximate Length of One degree

1. GENERAL APPEARANCE OF THE NEBULA ARGÛS AS SEEN NOV^{R}, 23RD & 24TH, 1868, WITH APERTURE 5 INCHES, AND MAGNIFYING POWER 55.

Malby & Sons, Lith.



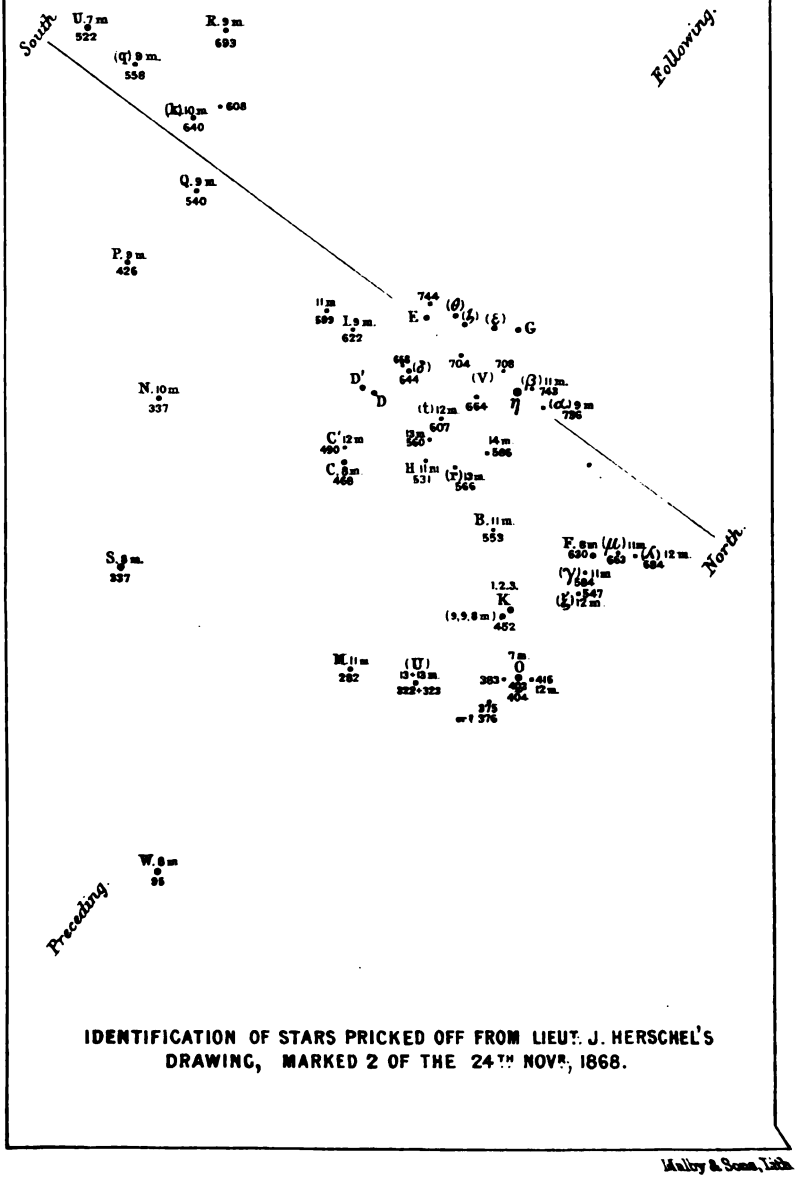
2. A PORTION OF THE NEBULA ARGÛS COPIED FROM SKETCHES AT THE TELESCOPE: APERTURE 5 INCHES: EYEPIECE 125.

J.H 24/11/68.

Malby & Sons, Lith

The Nebula round η Argûs, IV.

J.F.W.H. (A.A.).

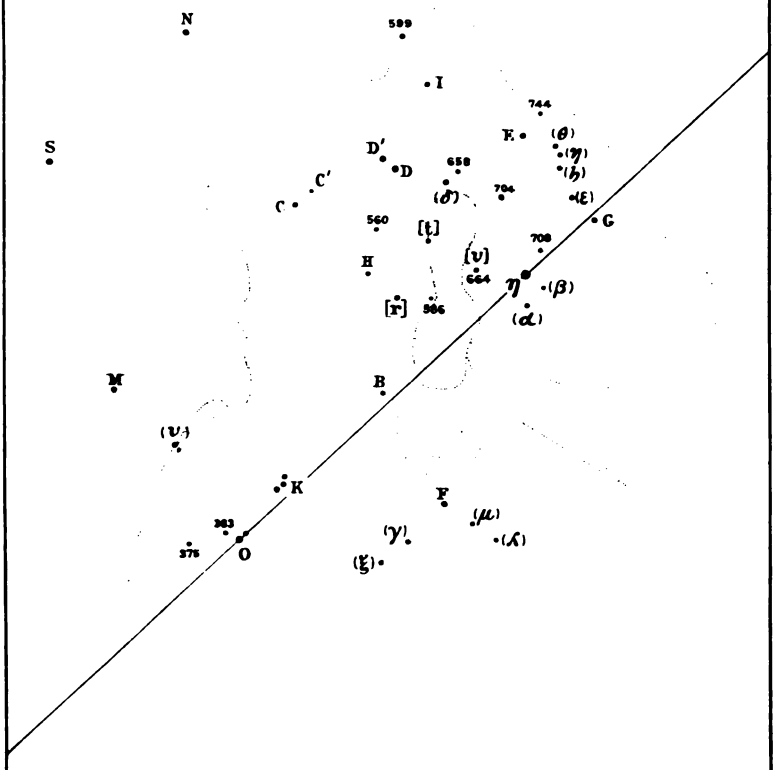


IDENTIFICATION OF STARS PRICKED OFF FROM LIEUT. J. HERSCHEL'S DRAWING, MARKED 2 OF THE 24TH NOVR., 1868.

Walby & Sons, Lith.

The Nebula round η Argûs, V.

J.F.W.H.(B.B.).



SEMIREVERSED BY PRICKING OFF FROM SIR J. HERSCHEL'S ENGRAVING OF THE NEBULA SHEWING THE TIME AND SITUATION OF THE IDENTIFIED STARS IN LIEUT. HERSCHEL'S SKETCH OF NOV^R. 24^TH., 1868, AND THE LEADING LINES OF THE NEBULA AMONG THEM, AS IT EXISTED IN 1834 — 1838.

The Oblique Line through η & O corresponds to the upright Sides of Lieut.^t, Herschel's diagram.

Malby & Sons, Lith.



without smoke or residue; this releases a detent, and a motion is thereby given to the hands of the clock, bringing it to the correct local mean time. There is a supplementary arrangement by which, if, in consequence of a passing cloud, the thread does not ignite at the instant of the transit of the first limb, the subsequent ignition of the thread will not affect the clock.

The Transit of Mercury of Nov. 4–5, 1868. By C. Todd, Esq.

(*Extract from a Letter to the Astronomer Royal.*)

The weather being fine and beautifully clear I was able to take the following observations of the transit of *Mercury* on the 5th instant, which may perhaps be useful for purposes of comparison. I had previously calculated the angle from vertex of ingress, and looked for the planet at the right point; the time of first contact was, however, late a few seconds. The time of internal contact at both ingress and egress may be taken as correct, although the Sun was low down and rather ill defined at the egress. The observations were taken with a good Dollond's telescope of $2\frac{1}{2}$-inches aperture.

Ingress.

	h	m	s	
a. First contact	2	35	20	Adelaide Mean Time.
b. Internal contact	2	41	30	,,

Egress.

c. Internal contact	6	13	53	,,
d. Planet bisected	6	14	59	,,
e. External contact	6	16	3	,,

a. Seen late. Sun's limb slightly indented.

b. Good.

c. Considered good, but limb uneven.

d. Only approximate.

e. Sun's limb seemed perfect, but time may be few seconds too soon, the Sun being low and ill defined.

Ellery only got ingress.

I have made a good galvanic determination of the difference of longitude between Adelaide and Melbourne. I have fitted contact springs to my transit clock, and converted an ink-writing Morse instrument, by Siemens, into a very good Chronograph.

Electric Telegraph Department,
Office of Superintendent,
Adelaide, Nov. 11, 1868.

Note on Mr. Huggins' paper "On a Possible Method of Viewing the Red Flames without an Eclipse." By J. Norman Lockyer, Esq.

In the paper referred to I read as follows:—

"The observations of the eclipse of August last having shown the position in the spectrum of the bright lines of the red flames, Mr. Lockyer and Mr. Janssen succeeded independently by a similar method in viewing the spectra of these objects."

The obvious meaning of the paragraph is that my work was based upon, and that my success was due to, the observations made during the eclipse in India.

I therefore beg permission to show (I.) that my work was not based upon the Indian observations, and (II.) that my success was in no way due to them.

I. In the first place I may remark that my work was begun in 1866, and the method I adopted was clearly indicated in a paper communicated to the Royal Society in that year. My observations were then unsuccessful owing to the small dispersive power of the instrument employed, which I have since ascertained was utterly incapable of showing the bright lines.

Secondly, I attempted to remedy this state of things by using an instrument of greater dispersive power, and I obtained a grant for the construction of such an instrument, which was ordered in January, 1867.

Thirdly, after a long series of unfortunate delays, due to instrument-makers and to my own severe illness, the instrument was received in an incomplete state on the 16th of October last (Friday night).

Fourthly, the discovery of the prominence spectrum was made on the 20th of October (Tuesday morning), *i.e.* four days after the instrument was received, and on the very first day I gave to the observations after the instrument had been mounted, adjusted, and tested.

Fifthly, my method of observation was to clamp my telescope on a particular part of the Sun's limb, and then to sweep along the spectrum very slowly (three adjustments being necessary) from red to violet, then to clamp on another part and work back, and then to begin again.

II. We now come to the second point. In what I have hitherto stated I have shown that the coincidence in time between my own results and the receipt of the information from India *was due to the date on which the new instrument was received.* I am convinced that had the new spectroscope been received a year ago the discovery would have been made just as easily; and Father Secchi's remarks on the ease of the observations, even with an instrument of moderate dispersive power, strengthen this view. I have also stated my method of work.

Now let us see how the Indian observations were the cause of my success, or if indeed they could have caused the success of any one who depended upon them.

On the 20th of October I knew of three sets of observations: Rayet's, Herschel's, and Tennant's.

Rayet gave	B	D	E	*b*	F
	and four more undetermined lines.				
Herschel gave	near B or C	D			near F
declaring against F, and hesitating very much to assign even an approximate place to the line in the red.					
Tennant gave	C	D		*b*	near F
	believed he also saw a line near G.				

The above table will, I think, show how utterly useless the Indian observations were for the purpose Mr. Huggins has assigned to them. In fact, every lettered line, except A and H, is named. Had I indeed worked with them at all I should naturally have looked for the line D, as that is the only line in which all the observations correspond. I distinctly referred to hydrogen (= C and F) in my paper of 1866, but in 1868 only one observer is sure of C, and only one observer is sure of F, and now it is stated that my success is due to the Indian observations.

I have sent this paper to the Royal Astronomical Society, not because any credit due to M. Janssen and myself is lessened by Mr. Huggins' paper, but because I know from experience that its *Monthly Notices* are one of the most important sources from which the history of Astronomy is written. I naturally therefore prefer that a true account of the recent discovery should appear in the *Monthly Notices*, instead of a statement incorrect in fact, which, were it left unnoticed, would mislead those who come after us.

On the Relation of the Luminous Prominences to the Faculæ of the Sun. By Prof. E. W. Brayley.

In the *Monthly Notices* for November 13, 1868, is inserted a paper on the Sun-spots and general aspect of the Sun on the day of the total eclipse of August 18, communicated by Messrs. De La Rue, Stewart, and Loewy. The large groups of spots visible, one near the eastern, the other near the western limb, were connected with faculous matter which apparently extended beyond the visible disk on both sides. The angles of position of these two parts having been given, the authors remark, "How far these

positions agree with any observed protuberances can only be gathered after some time, when the whole of the detailed accounts have come to hand."

Mr. De La Rue does not refer, on this occasion, to a similar observation of his own which is recorded in his Bakerian Lecture on the Eclipse of 1860. With the view of ascertaining whether any connexion exists between the luminous prominences and the faculæ, or the spots on the solar disk, he obtained photographs of the Sun on the only five days, from the 14th to the 20th of July, on which the state of the weather permitted.

The faculæ surrounding a certain group of spots "extended evidently," Mr. De La Rue states, "beyond the visible portion of the Sun's surface on the 18th; for a part which was not in sight on the 18th came into view on the 19th and 20th. Just in the neighbourhood of these faculæ there was visible in the telescope during the totality *a very brilliant sheet of light*."* The significance of this last fact will appear hereafter.

The consideration of the facts recorded in the paper by the Kew observers reminded me of some observations cited in a *resumé* of recent researches on the Sun contained in three papers on its physical constitution which I contributed to the *Companion to the Almanac* (Knight's) for the years 1864, 1865, and 1866. Among these are some of great importance in relation to the present subject.

We have evidence that the terms *luminous prominences* and *faculæ* merely designate different portions, the inferior and the superior respectively, of the same physical objects. For this we are principally indebted to the photographic researches of Mr. De La Rue, the results of which I shall proceed to interpret.

In the first of the papers just alluded to, inserted in the *Companion*, for the year 1864, p. 45, I stated that the prominences might be regarded with some probability as arising from the exterior flames and condensed vapour or smoke "of the faculæ or their crests;" though I had not then recognised the force of the evidence on the subject which existed even at that time.

Mr. De La Rue had inferred from his own observations, both telescopic and photographic, of the prominences as seen in 1860, that they are far more generally distributed on the solar disk than the spots, being indeed very widely scattered over the Sun's surface. I afterwards stated (*Ib.*) "We must infer from the great extent of the limb they occupy, in profile or section, that they extend over the whole surface of the Sun." I did not mean as a continuous stratum (to which idea I have no claim), but merely as a collection of detached clouds, as they were then assumed to be, or detached ranges of clouds in the same plane as it were, forming a discontinuous spherical shell above the photosphere and the absorptive composite atmosphere incumbent upon it.

* *Phil. Trans.* 1862, pp. 395–396.

By placing photographic pictures of portions of the Sun's surface in the stereoscope, Mr. De La Rue ascertained that the faculæ, as he inferred, occupy the highest positions of the photosphere; and "in one case," he relates, "parts of the faculæ were discovered to be sailing over a spot, apparently at some considerable height above it."*

Now the first of these results agrees with the fact that the luminous prominences are the most exterior phenomena of the Sun which are seen upon or above the limb; the second exactly describes the appearance which a floating prominence would have in a photograph viewed by the stereoscope; the actinic power of the prominences in general having been found by Mr. De La Rue to be very great. The prominences are the only phenomena of the Sun otherwise known which are seen detached from the edge of the disk. This result agrees also with the facts that some of the prominences are detached, and at a distance from the edge of the disk equivalent to the altitude of many thousand miles above the photosphere, which we know to be attained by many of the prominences which appear to be still connected with the edge.

I have stated, in the first article on the Sun already referred to, after Mr. De La Rue, that he entertains the hope of obtaining the outline of the prominences ("in plan, as it were, in solar horizontality," I have added for the sake of perspicuity), "'as very delicate dark markings,'" now to use his own words, "'on the more brilliant mottled background of the photosphere,' which, by the use of the stereoscope, will be discriminated from the other markings of the Sun's surface, and appear in their true position." It is remarkable that Mr. De La Rue did not recognise the fact that the photographic observation of the floating facula already cited was in reality an example of the fulfilment of this hope. What other appearance could one of the detached luminous prominences when thus observed present? It might indeed have been continuous with the spot over which it was seen, if the intervening matter were transparent, and that this was actually the case is manifest from the observation itself.

We have thus the strongest evidence that the faculæ and the luminous prominences are identical, or at least that the latter are the superior terminations of the former. If such be the fact, they must also be connected with the spots. I shall endeavour to evince, in another paper, that it will be consistent with all known facts to believe that they are the prolonged summits, rising from the level of the photosphere, of the torrents of matter originally wholly gaseous, ascending from the nucleal regions of the Sun, which, piercing through the successive envelopes of the nucleus from the cloudy stratum to the photosphere, cause the sun-spots; that the observed height to which the prominences ascend is at once the consequence and the index of the enormous force with which the original torrents are projected from the nucleus; and

* *Phil. Trans.* 1862, p. 407.

that these inferences are not inconsistent with the local character of the spots.

London Institution, Jan. 8, 1869.

Addition, January 23, 1869.—Since the reading of this paper, it has been objected that, in consequence of the brilliancy of the photosphere exceeding by some hundreds of times that of the luminous prominences, while it is itself exceeded by that of the faculæ, the phenomenon observed by Mr. De La Rue—"the bright appendage of the Sun," which sailed "over the spot"—could not have been a prominence.

In the paper above announced, however, I shall adduce reasons, founded on Mr. De La Rue's own data, combined with the evidence of the spectroscope, for believing that in the stereoscopic projection of photographic pictures of the Sun's disk, the prominences, notwithstanding their inferior intrinsic brilliancy, may be expected to be seen as *bright* and not as *dark* markings, in agreement with my interpretation of the photographic appearance in question.

It has also been objected that the luminous prominences cannot be even partially identical with the faculæ, because the continuous spectra of the latter, forming part of the solar spectrum, proves them to consist, not of aëriform, but of solid or liquid materials (probably] in the state of incandescent cloud), while the prominences have now been shown to be simply gaseous.

To this I shall only reply, at present, that, at the temperature of the photosphere, the faculous matter (especially if it consists of solid or liquid hydrogen), must be perpetually undergoing resolution into the aëriform condition at its upper surface, and, therefore, that while the spectrum of the lower parts of the faculæ-prominences will be continuous, that of their upper portions will be monochromatic, in accordance with the observed facts. But to this subject also I shall return in the sequel.

A communication has been received from Comm. D. E. Ashe, Director of the Quebec Observatory, stating that, since the date of his paper, "On the Physical Constitution of the Sun," *Monthly Notices*, vol. xxvi., p. 61, he had had his Equatoreal (aperture 8 inches, focal length 9 feet) fitted for Celestial Photography by merely inserting a sheet iron tube, in which is fixed the tube of a "Voigtländer" Camera, giving a picture of the Sun 3·4 inches in diameter, at a distance of 14 feet from the object-glass, and that the photographs are very good — two negatives sent to the Astronomer Royal showing all the phenomena of the surface, even the willow-leaves or granulations. The author's observations have confirmed him in the opinion, and he adduces further arguments

in support of the theory that the Sun-spots are planetary bodies of small dimensions, originally revolving between *Mercury* and the Sun, and which have fallen upon the Sun.

Comparisons of Sun-spot Observations made in Dessau and Kew Observatory in the year 1868.

Communicated by Messrs. De La Rue, Stewart, and Loewy.

Months.	Dessau.					Kew Observatory.				
	Number of New Groups.	Numbers of the Groups.		Days of Obs.	Days without Sun-spots.	Number of New Groups.	Numbers of the Groups.		Days of Obs.	Days without Sun-spots.
January	2	No. 1 to	No. 2	19	10	2	No. 787 to	No. 788	6	4
February	2	3	4	21	2	5	789	793	9	2
March	8	3	12	27	0	8	794	801	12	0
April	8	13	20	25	0	6	802	807	12	0
May	7	21	27	31	1	5	808	812	21	0
June	9	28	36	28	2	15	813	827	19	2
July	8	37	44	31	7	8	828	835	21	4
August	8	45	52	31	0	7	836	842	16	0
Sept.	13	53	65	30	1	13	843	855	12	0
Oct.	13	66	78	28	0	17	856	872	16	0
Nov.	11	79	89	19	0	8	873	880	7	0
Dec.	12	90	101	11	0	21	881	901	13	0
Total	101	No. 1 to	101	301	23	115	No. 787 to	901	164	12

REMARKS.

The numbers of the groups in the Kew list are in continuation of the Catalogue given by Messrs. De La Rue, Stewart, and Loewy, in their "Researches on Solar Physics," part i.

The cause of the discrepancy in the total result is the discontinuation of the observations in Dessau since December 15, in consequence of the illness of Hofrath Schwabe. The latter part of December was especially rich in new and large groups, and there is every reason to believe that we are approaching rapidly the Sun-spot Maximum.

A very interesting phenomenon, viz. an indentation in the Sun's limb produced by a spot V, group No. 807, was for the first time simultaneously observed by the Kew and Dessau observers on May 7. The phenomenon was discussed by Messrs. De La Rue, Stewart, and Loewy, and the discussion has been ordered to be printed in the *Transactions of the Royal Society*.

Kew Observatory,
Jan. 7, 1869.

On the Nebular Hypothesis, and the Approximate Commensurability of the Planetary Periods. By Daniel Kirkwood, LL.D. Professor of Mathematics, Indiana University.

The views of Laplace in regard to the development of our planetary system have been confirmed in a remarkable manner by the recent progress of astronomical discovery. We have now (1868) 100 minor planets between *Mars* and *Jupiter*, of which 96 have been detected since 1845. The mean distance of *Flora*, the innermost member of the group, is 2·20; that of *Sylvia*, the most remote, 3·49. The breadth of the zone is, therefore, greater than the distance of the Earth from the Sun; greater even than the entire interval between the orbits of *Mercury* and *Mars*. Moreover, the *perihelion* distance of *Sylvia* exceeds the *aphelion* distance of *Harmonia* by a quantity equal to the interval between the orbits of *Mars* and the Earth. The Olbersian hypothesis of the origin of these bodies seems thus to have lost all claim to probability. The only remaining theory worthy of consideration is that of Laplace, proposed in 1813, soon after the discovery of the first member of the group. "The double fact," says Plana, "of the multitude of those bodies and of their circulation in the same direction around the Sun, is now too imposing to admit an explanation of their origin and formation different from that developed by Laplace, in his *Système du Monde*." *

The known facts, however, in regard to the zone of the minor planets, as well as the phenomena of *Saturn's* rings, seem to demand a modification of the Nebular Hypothesis as generally held. No reason has ever been assigned why the solar nebula should not have abandoned rings at distances intermediate between the present orbits of the planets. On the contrary, it seems highly probable that, after first reaching the point at which gravity was counterbalanced by the centrifugal force arising from the rotation of the contracting spheroid, a continuous succession of narrow rings would be thrown off in close proximity to each other, and revolving in different periods according to Kepler's third law. It would seem, moreover, as shown by Mr. Trowbridge,† that the solar nebula, during the process of ring formation, was very oblate, and also much condensed at the centre; while, according to Prof. Ennis,‡ the rotation of the nucleus was extremely slow.

The theory of an almost continuous separation of matter at

* *Note sur la Formation probable de la Multitude des Asteroïdes qui, entre Mars et Jupiter, circulent autour du Soleil.* Par Jean Plana. Présentée le 2 Mars, 1856, à l'Académie Royale des Sciences de Turin.

In a subseqnent paper, by the same distinguished astronomer, it was shown that the mean inclination of the 62 minor planets known in 1861 was 8° 25′; exceeding the mean inclination of the solar equator to the ecliptic (7° 9′) by only 1° 16′. This coincidence has been rendered still more close by later discoveries; the mean inclination of 100 asteroids being almost exactly 8°.

† Silliman's *Journal*, Nov. 1864, pp. 350–353.

‡ *Origin of the Stars*, section xix.

the equator of the solar spheroid is sustained by the fact that the zone of minor planets has evidently resulted, not from a single primitive ring, the parts of which moved with nearly the same angular velocity, but from *an indefinite number of narrow annuli*. The rings of *Saturn* may be regarded as another striking index to the process of planetary formation. With the exception of two permanent intervals, they appear to consist of small discrete masses, revolving in different periods according to their respective distances.

Why did not the Asteroid-zone form a single planet, and the rings of Saturn, one or more satellites? — In regard to the ring between *Mars* and *Jupiter*, perhaps no satisfactory answer can yet be given. We may remark, however, (1), that being situated just within the orbit of *Jupiter*, the perturbations were greater than in any other part of the system; and (2), that, as the total mass of the asteroids is very small, the matter of the primitive annulus was extremely rare, so that the intersection of orbits, resulting from the perturbations, would be less likely to produce large planetary nuclei. The rings of *Saturn* were doubtless comparatively dense. It has been shown, however, by Professor Vaughan,* that in positions so near the primary the force of gravity would render satellites of any considerable magnitude unstable. This force alone would, therefore, prevent the collection of large masses in the planetary form near the central body.

As these parts of the solar system, viz. the rings of *Saturn* and the minor planets between *Mars* and *Jupiter*, appear to furnish strong arguments in favour of the nebular hypothesis,† their phenomena may perhaps be further suggestive as to the *mode* of planetary formation.

The mean distance of a planet having—

A period equal to	$\frac{1}{2}$	that of Jupiter is	3·2776
,,	$\frac{4}{9}$	,,	3·0299
,,	$\frac{3}{7}$	,,	2·9574
,,	$\frac{2}{5}$	,,	2·8245
,,	$\frac{1}{3}$	,,	2·5012
,,	$\frac{2}{7}$	,,	2·2569

These distances all fall between the greatest and least mean distances of the asteroids. Now, do we find wider intervals in these portions of the ring than elsewhere? and if so, what is their physical cause? For the purpose of comparison we have, in the following table, arranged the minor planets in the order of their mean distances from the Sun.

* *Proc. Amer. Assoc. for Ad. of Sci.* 1856, p. 111.

† "No part of the solar system affords so striking an argument in favour of Laplace's Nebular Hypothesis as the Saturnian system of rings."—Proctor's *Saturn and its System*, p. 201. Plana's views in regard to the asteroids have been already quoted.

Order of Disc.	Name.	Distance.	Order of Disc.	Name.	Distance.
(8)	Flora	2·2014	(78)	Diana	2·6228
(48)	Ariadne	2·2034	(28)	Thalia	2·6250
(71)	Feronia	2·2654	(87)	Fides	2·6422
(40)	Harmonia	2·2677	(16)	Eunomia	2·6429
(18)	Melpomene	2·2956	(51)	Virginia	2·6486
(80)	Sappho	2·2971	(66)	Maia	2·6539
(19)	Victoria	2·3342	(26)	Proserpine	2·6556
(27)	Euterpe	2·3468	(85)	Io	2·6594
(4)	Vesta	2·3613	(72)	Clytie	2·6648
(30)	Urania	2·3642	(97)	Clotho	2·6661
(24)	Clio	2·3674	(3)	Juno	2·6687
(52)	Nemausa	2·3779	(75)	Eurydice	2·6698
(9)	Metis	2·3862	(77)	Frigga	2·6719
(7)	Iris	2·3862	(54)	Angelina	2·6783
(22)	Echo	2·3939	(34)	Circe	2·6839
(68)	Ausonia	2·3972	(58)	Concordia	2·6979
(25)	Phocea	2·4023	(55)	Alexandra	2·7076
(20)	Massilia	2·4093	(50)	Elpis	2·7147
(97)	Asia	2·4209	(45)	Eugenia	2·7159
(44)	Nysa	2·4242	(93)	Leda	2·7399
(6)	Hebe	2·4254	(96)	Atalanta	2·7487
(88)	Beatrix	2·4287	(93)	Minerva	2·7554
(21)	Lutetia	2·4354	(72)	Niobe	2·7555
(42)	Isis	2·4401	(92)	Alcmene	2·7603
(19)	Fortuna	2·4414	(1)	Ceres	2·7660
(79)	Eurynome	2·4433	(41)	Daphne	2·7674
(11)	Parthenope	2·4526	(88)	Thisbe	2·7687
(17)	Thetis	2·4737	(56)	Pandora	2·7692
(46)	Hestia	2·5178	(2)	Pallas	2·7700
(89)	Julia	2·5498	(81)	Lætitia	2·7710
(29)	Amphitrite	2·5544	(69)	Leto	2·7748
(91)	Ægina	2·5584	(74)	Galatea	2·7777
(0)	Astræa	2·5772	(98)	Bellona	2·7784
(13)	Egeria	2·5775	(81)	Terpsichore	2·8563
(32)	Pomona	2·5831	(33)	Polyhymnia	2·8646
(14)	Irene	2·5895	(48)	Aglaia	2·8831
(47)	Melete	2·5976	(22)	Calliope	2·9091
(54)	Calypso	2·6101	(16)	Psyche	2·9263
(70)	Panopea	2·6132	(60)	Danae	2·9746

Order of Disc.	Name.	Distance.	Order of Disc.	Name.	Distance.
(88)	Hesperia	2·9949	(10)	Hygeia	3·1494
(85)	Leucothea	3·0060	(81)	Euphrosyne	3·1561
(96)	Ægle	3·0549	(90)	Antiope	3·1576
(36)	Arethusa	3·0829	(87)	Mnemosyne	3·1597
(80)	Pales	3·0861	(94)	Aurora	3·1629
(52)	Europa	3·0999	(92)	Undina	3·1917
(49)	Doris	3·1044	(76)	Freia	3·3877
(86)	Semele	3·1119	(65)	Cybele	3·4205
(61)	Erato	3·1309	(97)	Sylvia	3·4927
(24)	Themis	3·1420			

Remarks on the foregoing Table.

The mean between the distances of *Flora* and *Sylvia* is 2·8470; and as small bodies in the remoter part of the ring are more difficult of detection, the zone will be considered under two divisions. The inner section contains 72 of the 97 asteroids whose elements are known; the mean interval between them being 0·0081. The greatest gap in the order of distances occurs between *Ariadne* and *Feronia*. It is 0·0620, or nearly eight times the mean. This includes the distance at which seven periods of an asteroid would be equal to two of *Jupiter*. The chasm next in order of breadth is between *Thetis* and *Hestia*. This is 0·0441, or more than five times the mean. In the outer section the mean interval is 0·0286. The greatest hiatus is between *Undina* and *Freia*; the breadth being 0·1960, or more than eight times the mean. This includes the distance at which two periods of a planet would be equal to one of *Jupiter*. The second is between *Bellona* and *Terpsichore* (0·0779), and the third between *Leucothea* and *Ægle** (0·0489). The widest chasms in the zone of minor planets are thus found to include those distances at which the periodic times would be commensurable with that of *Jupiter*. These coincidences are not accidental. The *primitive* ring undoubtedly contained nebulous matter at these as well as the intervening distances; and the present existence of such intervals may be accounted for as follows:—

A planetary particle at the distance 2·5 — in the interval between *Thetis* and *Hestia*—would make precisely three revolutions while *Jupiter* completes one; coming always into conjunction with that planet in the same parts of its path. Consequently, its orbit would become more and more eccentric until the particle would unite with others, either interior or exterior, thus forming the nucleus of an asteroid. Even should the disturbed body not

* We here except the most remote interval, that between *Cybele* and *Sylvia*, which is 0·0722.

come in contact with other matter, the action of *Jupiter* would ultimately change its mean distance, and thus destroy the com mensurability of the periodic times. In either case the primitive orbit of the particle would be left destitute of matter. The same reasoning is, of course, applicable to other intervals.

The Rings of Saturn.

The equatoreal radius of *Saturn*, and the dimensions of the two bright rings, according to the measures of Struve,* De La Rue,† Main,‡ and Jacob,§ together with the mean of these measures, are as follows:—

	Struve.	De La Rue.	Main.	Jacob.	Mean.	In Equatoreal Rad. of Primary.
	″	″	″	″	″	″
Equatoreal Radius of Planet ..	8·9955	8·83	8·78	8·95	8·8864	
Exterior Radius of Outer Ring	20·0475	19·915	19·865	19·995	19·9556	2·2456
Interior ,, ,,	17·6445	17·665	..	17·910	17·7398	1·9963
Exterior Radius of Inner Ring	17·2375	16·725	..	17·425	17·1292	1·9276
Interior ,, ,,	13·334	13·455	13·825	13·135	13·4372	1·5121

	h	m	s
The period a Satellite revolving at the distance 1·9276, the interior limit of the interval, is	10	52	11
One-sixth of the period of Dione is	10	56	53
One-third ,, Enceladus	10	59	22
One-half ,, Mimas	11	18	32
One-fourth ,, Tethys	11	19	36
And the period of a Satellite at the distance 1·9963, the exterior limit of the interval, is	11	35	18

It is thus seen that the interval occupies precisely the space in which the periods of satellites would be commensurable with those of the four members of the system immediately exterior. As, therefore, the powerful attraction of *Jupiter* produces the observed *gaps* in the asteroid zone, so *the disturbing influence of Saturn's interior satellites is the physical cause of the permanent interval between the two bright rings.*

These facts and considerations are suggestive in regard to the numerous instances of approximate commensurability in the planetary periods. Let us suppose, in the first place, that at a former epoch the two innermost satellites of *Saturn*, *Mimas*, and *Enceladus*, constituted a broad, flat, and nearly continuous nebular ring, the separate portions of which revolved in different periods;

* *Mem. Ast. Soc.* vol. iii. p. 301.
† *Monthly Notices*, vol. xvi. p. 43.
‡ Ib. p. 35. § Ib. p. 124.

the matter of the exterior satellites having been previously collected into planetary forms. The first four satellites have the following periods:—

	d	h	m	s
Mimas	0	22	37	28
Enceladus	1	8	53	7
Tethys	1	21	18	26
Dione	2	17	41	9

The ring, therefore, from which the two innermost satellites were (hypothetically) derived must have contained (1) the orbit of a satellite whose period would be $22^h\ 39^m\ 13^s$, or one half that of *Tethys;* (2), that of a body revolving in $21^h\ 53^m\ 43^s$, or one third that of *Dione;* (3), that corresponding to a period of $1^d\ 8^h\ 50^m\ 34^s$, or one-half that of *Dione;* and (4), that of a mass whose period would be $1^d\ 6^h\ 12^m\ 17^s$, or two-thirds that of *Tethys.* The portions of this annulus at the distances named would be *unstable*, like the original matter in the interval of the present ring. The first two distances are nearly the same with that of *Mimas;* the third and fourth with that of *Enceladus.* The collision of masses and the formation of nuclei near the present orbits of these satellites would, therefore, result from the disturbing influence of *Tethys* and *Dione.* The remoter satellites afford similar instances illustrative of the same idea. Still others might be specified, but they will readily occur to every astronomer.

The degree of eccentricity attained by the orbit of such matter previous to its union, near one of the apsides, with other portions of the zone, would probably depend on the density of the original nebulous ring. This, in the case of the ring between *Mars* and *Jupiter*, is known to have been very small; and, accordingly, we find here the greatest excentricities. It is obvious, moreover, that the nearest approximations to commensurability would generally be found in cases where the density of the primitive rings was the greatest.

The instances of nearly commensurable periods in the Jovian and Saturnian systems, as well as among the primary planets, have long been well known, and are too numerous and remarkable to be accidental coincidences. That the physical cause of such phenomena is a legitimate object of research will hardly be called in question. Hitherto, however, no explanation of the facts has been even attempted.

The foregoing view of the Nebular Hypothesis may not be necessary to account for the transmutation of rings into the planetary form. It assigns, however, an obvious cause for the establishment of nuclei in such positions that their periods will be nearly commensurable with that of the disturbing body. As these nuclei would receive accretions of matter from portions of space both interior and exterior to their respective orbits, their

distances from the central body, during their planetary growth, would not be liable to great variation.

On Aboul Hhassan's Catalogue of 240 *Stars.*

By E. J. Stone, Esq.

The interest attaching to our few ancient Catalogues of stars, is, perhaps, sufficiently great to excuse my calling the attention of the Society to a Catalogue of 240 stars, contained in the work of Aboul Hhassan Ali, translated from the Arabic by J. J. Sédillot. Paris, 1834.

The longitudes and latitudes are given for the commencement of the Hegira, July 15, A. D. 622. It is not known, I believe, from what sources these observations were drawn. Delambre, *Astronomie du Moyen Age*, p. 186, merely says, "Il nous donne un catalogue de 240 étoiles, pour le commencement de l'hégire, c'est-à-dire pour le 15 Juillet, 622. Nous l'avons réduit à l'époque actuelle; il nous a paru trop inexact pour être reproduit ici; les erreurs les plus ordinaires passent un degré. On y voit quelques étoiles australes que ne sont point dans le catalogue de Ptolémie." And, again, on the same page,—"Il donne les déclinaisons de 180 étoiles, pour le commencement de l'hégire, sans nous dire comment ces déclinaisons ont été observées. On peut croire qu'elles sont tirées des ouvrages d'Arzachel."

I find more than 200 of these stars in Ptolemy's Catalogue. I have diminished all the longitudes of Hhassan's Catalogue by 6° 40′, and then compared the latitudes and diminished longitudes with the corresponding latitudes and longitudes of Ptolemy. The agreement is very close. The latitudes are identical; except in a very few cases indeed, and in nearly all these cases of apparent exception, one at least of the different readings given by Baily will be found to agree with the latitude of Hhassan's Catalogue. Ptolemy's longitudes are only given to the nearest 10′.

In some cases Hhassan's longitudes, as given by Sédillot, end with a 7′, 8′, or 9′. In these cases we have respectively a discordance of 3′, 2′, or 1′ between Hhassan's reduced longitude and that of Ptolemy. In nearly every other case we have identical results. The agreement, moreover, between the two Catalogues is such that in three or four cases where Ptolemy's longitudes are evidently 2° or 3° in error, relatively to the other stars, the same error is reproduced in Hhassan's Catalogue. I may mention that in the greater number of cases of discordance between the different versions of Ptolemy quoted by Baily, Hhassan agrees with the copy marked L, "Venice edition, in Latin, by Liechtenstein, 1515," which is known to have been derived from Arabic sources. I am unable to account, in a manner perfectly satisfactory to myself, for the appearance of the slight discordances of 1′, 2′, or 3′,

of which the discordance of 2′ is by far the most common. These discordances are far too small to express the errors of observations of two independent catalogues. This will appear at once from a comparison between Ptolemy's Catalogue and that of Ulugh Beigh.

It has occurred to me that Ptolemy's places may have been laid down upon a globe and subsequently reproduced, for, perhaps, another epoch, without attention to the restriction of always taking the longitudes to the nearest 10′. This would give rise to small discordances such as really exist between the catalogues. Whatever may be the true explanation of these small differences, I have not the slightest doubt about Hhassan's Catalogue being mainly a reproduction of that of Ptolemy.

There is one point connected with this Catalogue which appears to me to throw great light upon the origin of the belief which existed amongst many of the Arabic astronomers, of an oscillation in the precessional motion. Hhassan gives a Table for the calculation of the precessional motion in longitude in any time. His Table is founded upon the assumption:

$$\text{Precessional motion} = \text{Constant} \times \sin (5'{\cdot}409\, t + 3^{\circ}{\cdot}51')$$

where t is the number of Arabic years from the commencement of the Hegira.

Hhassan's Table has been computed with natural sines to only two places of decimals, and these not always correct in the second place. It is therefore rather difficult to see the exact value of the constant which has virtually been employed. If, however, we take 610″ for the value, the results agree closely with those given by Hhassan.

Hhassan quotes an observation of Hipparchus and another of Arzachel to show the accuracy of his result. He does not mention Ptolemy's Catalogue.

If we compute from the formula the precessional motion from the commencement of the Hegira to the epochs of the Catalogues of Arzachel, Ptolemy, and Hipparchus, we have

For Arzachel's Catalogue	t =	+ 473
Ptolemy's do.	t =	− 500
Hipparchus's do.	t =	− 775

Hence from the formula

		°	′
Precessional motion to Arzachel's time	=	+ 7	22
Ptolemy's do.	=	− 6	42
Hipparchus's do.	=	− 9	17

Hence between Hipparchus and Arzachel we have a motion of 16° 39′. Between Hipparchus and Ptolemy it is 2° 35′. Assuming the uniform precession equal to 50″, we have for the increase of

longitude between Hipparchus and Arzachel, 16° 45′, agreeing very closely with that given by the formula. The precessional motion between Hipparchus and Ptolemy agrees with that assumed by Ptolemy, viz., 2° 40′. Hence the origin of Hhassan's formula appears to me clear: he could not reconcile the three great catalogues of Hipparchus, Ptolemy, and Arzachel, on the assumption of an uniform change of longitude. He adopted a simple formula which would reconcile these three catalogues, and, not doubting the accuracy of the observations, assumed that this formula must express a true law of nature.

If this view be correct, his belief in the oscillatory motion of the first point of *Aries* arose entirely from the systematic error—of about a degree—in Ptolemy's longitudes; and Ptolemy's want of candour respecting the nature of his Catalogue, is thus found to be throwing astronomy into complexities more than 1100 years after his death.

Measures of ζ Herculis. By G. Knott.

During the past summer I obtained the following measures of the binary star *ζ Herculis* with my 7½-in. Alvan Clark refractor, and a parallel wire micrometer by Dollond.

P=204°·46	obs. 5	w. 23	D=0″·951	obs. 4	w. 8	Epoch 1868·398
206·95	5	23	1·088	4	16	·452
208·19	6	33	1·025	4	22	·474
205·90	5	27	0·896	4	20	·606

Mean results, allowing weight,—

P=206°·53	obs. 21	w. 106	D=0″·992	obs. 16	w. 66	Epoch 1868·48

Considerable pains were bestowed on the distance measures from the fact that with the full aperture of 7·33 inches the image of the small star was projected almost centrally upon (perhaps very slightly *within*) the first bright ring surrounding the primary, thus affording a favourable opportunity for determining the radius of the first ring with that aperture. The result is in most close accordance with that given by theory. The radius of the first bright ring for a telescopic aperture of 7½-in. calculated from the numbers given by the Astronomer Royal on p. 80 of his *Undulatory Theory of Optics*, being 1″·009, which differs by only 0″·017 from the mean result of my own measures.

Woodcroft Observatory, Cuckfield, Sussex,
December 10, 1868.

Occultations of Stars by the Moon, observed at the Durham Observatory, 1867, November, 1868, May. By J. L. Plummer.

(*Communicated by Prof. Chevallier.*)

Day of Obs.	Phenomena.	Moon's Limb.	G.M.T. h m s
1867. Nov. 6	Disapp. of λ Aquarii	Dark	10 24 57·5
	Reapp. of λ Aquarii	Bright	11 33 52·2
	Disapp. of 78 Aquarii	Dark	11 57 18·6
Nov. 8	Disapp. of 10 Ceti	Dark	7 10 6·1
	(*a*) Reapp. of 10 Ceti	Bright	8 23 8·1
Dec. 11	(*b*) Disapp. of 130 Tauri	Bright	8 47 38·9
	(*c*) Reapp. of 130 Tauri	Dark	9 33 24·3
Dec. 16	Disapp. of *c* Leonis	Bright	13 54 23·9
	Reapp. of *c* Leonis	Dark	15 1 56·8
1868. Feb. 4	Disapp. of 130 Tauri	Dark	6 55 8·6
Mar. 28	(*d*) Disapp. of γ Tauri	Dark	9 1 4·9
	Reapp. of γ Tauri	Bright	9 56 3·6
Apr. 9	(*e*) Disapp. of η Libræ	Bright	13 30 12·7
	(*f*) Reapp. of η Libræ	Dark	14 6 15·3
May 4	Disapp. of τ^2 Virginis	Dark	9 6 45·9
	Reapp. of τ^2 Virginis	Bright	10 17 42·5

(*a.*) Not exact; the Moon's limb unsteady. (*b.*) Not very exact; the star being faint from haze and the Moon only a few hours past the full. (*c.*) Not good; the star faint. (*d.*) The star was attached to the Moon's limb, which was distinctly visible by earthlight, at least 5 seconds before the disappearance, which was instantaneous. (*e.*) The star faint; the observation not very satisfactory. (*f.*) Light clouds passing, but the observation pretty good. (*g.*) Uncertain; the Moon's limb very unsteady.

Durham Observatory.

On a Defective Vision. By Major Tennant, R.E.

Some few members of the Royal Astronomical Society may be aware that I have occasionally complained of a somewhat defective vision. If I venture to enter into some details about it, it is not that the matter is of much importance as regards myself, but that I think many more are labouring under slight defects, and being unconscious of them, attach, in perfect good faith, a value to delicate observations which they do not deserve.

Many years ago, soon after I came to India, I found in look-

ing at the Snowy Ranges of the Himmaleh with a telescope that to my eye the snow had a reddish-brown tint. Shortly after I found that this was not the same for both eyes, but that, in passing from my right eye to my left with a telescope, I seemed to pass, as it were, from the brownish tints of autumn to the brilliant ones of spring. Every object became brighter and clearer, and the greens were especially affected. Close examination showed me that the vertical and horizontal wires in a telecope were never pleasantly seen together. One was always clearer than the other, and any occasional perfect vision passed away after the effort to see wore off.

I came to the conclusion then that there was really some defect in my eye. In 1857 and 1858 I was examining many of the double stars in Smyth's *Cycle*, and it struck me that I could not see blue stars two magnitudes larger than others there described which I did see. I also noticed that the very red stars described had never produced on me any marked impression of colour, and I deduced that my eyes were really somewhat insensitive to the blue rays.

So matters stood till last December, when I accidentally saw an optometer at Messrs. Horne and Thornthwaite's, which led me to ask Mr. Acland of that firm to examine my eyes. He came to the conclusion that I was somewhat short-sighted, and that the lenses of the eye did not refract as solids of revolution, but that a plano-cylindrical convex lens of 30 inches radius would nearly adjust my vision. I tried these, and, after finding the proper positions of the axes of the cylinders, found that the definition was enormously improved, and that the left eye was, as I thought, nearly perfect, and the right eye nearly as good as the left originally, but I was firmly convinced for some time that I was *not* short-sighted. At last, however, I became convinced that I was so with these lenses, and a spherical surface of the same radius as the cylinder was ground inside it. Now my vision was superior to anything before. On the voyage out I have often seen eight stars of the *Pleiades* (and notes of their places are in a note-book) where I never before saw more than five; but I found now I had become long-sighted, and could not use the glasses for reading, and became convinced that Mr. Acland's statement was correct, namely, that I was short-sighted, not because the near focus was short, but because I had a want of the usual range of adjustment and could not reach the distant one; when, therefore, distant vision was corrected the near vision became defective. I therefore made up my mind to have two sets of lenses.

But before ordering them I determined to re-examine my eyes with great care, and I now found, on using an achromatic glass as an eye-glass, and a peculiar object, that, in reality, the estimate I had made of the improvement in my right eye was very nearly right. I now found that the convex surface for the right eye required a radius of 20·5 inches, and for the left eye

one of 40, and that the concavity for distant vision required to be 27·5 inches in radius for each.

The red tint now remained to be accounted for, and I have been led to what I believe to be the true solution of that, and also of the insensibility of the right eye to blue light. I should mention that habit makes me see mainly with the right eye, that is, that its image clearly predominates in ordinary vision. I had noticed that while my general vision of stars was much improved with the spectacles I had brought from England, and the sky became dotted with small objects invisible without them, some stars certainly were less clear. It only occurred to me recently that on that occasion *α Scorpii* was injured and *Saturn* improved in definition by the glasses, and I then remembered that *α Scorpii* had generally been the defaulter, the extreme beauty of that constellation having always made it catch my eye. It is evident *now* that my right eye is not achromatic, that the red rays being naturally focussed predominate in vision, and that when the luminous image of a star is focussed the blue rays do not claim notice, but when a blue star is seen, if it be so small as to be near the minimum of brightness, its image is too diffused to be visible.

Now, my vision I used to think good. I have often at sea caught the masts and rigging of a ship before those searching for them, and the defects which I have spoken of I have frequently believed to be fancies, as they seemed inconsistent with many evidences that I had good vision. I should certainly have unhesitatingly trusted my eyes in any matter but colour. Possibly some of the strange discrepancies in description are due to similar defects of vision, and we may, some day, find it necessary to make every observer of delicate phenomena, who claims credit, *prove that he can see.*

Calcutta, Oct. 15, 1868.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *February* 12, 1869. No. 4.

Admiral MANNERS, President, in the Chair.

W. J. H. Beechey, Esq., Westbury-on-Severn;
Jas. Robert Jones, Esq., Aberdeen;
John Lee, Esq., St, Peter's Chambers, Cornhill;
Dr. Geo. Royston Pigott, 2 Lansdowne Crescent, Notting Hill; and
T. Ryle, Esq., Eastbourne,

were balloted for and duly elected Fellows of the Society.

Report of the Council to the Forty-ninth General Meeting of the Society.

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patrones, and Honorary.	Total Fellows.	Associates.	Grand Total.
December 31, 1867	179	286	13	4	482	46	528
Since elected ...	5	18	...	...	...	...	...
Deceased	−5	−5	−1	−1	...	−1	...
Removals	+3	−3	...	...	...	...	...
Dec. 31, 1868 ...	182	296	12	3	493	45	538

Mr. Whitbread's Account as Treasurer of the Royal Astronomical Society, from January 1 to December 31, 1868:—

RECEIPTS.

	£	s.	d.	£	s.	d.
Balance of last year's account	...		...	117	18	9
By Dividend on £2800 Consols	40	19	0			
By ditto on £5000 New 3 per Cents ...	73	2	6			
By ditto on £2800 Consols	40	19	0			
By ditto on £5000 New 3 per Cents ...	73	2	6			
				228	3	0
On account of arrears of contributions ...	141	10	0			
182 annual contributions	382	4	0			
22 admission-fees	46	4	0			
17 first years' contributions	26	5	0			
				596	3	0
7 compositions	...		...	144	18	0
Sale of publications:—						
At the Rooms of the Society	79	8	0			
By Messrs. Williams and Norgate, Publishers	42	9	0			
				121	17	0
				£1208	19	9

EXPENDITURE.

	£	s.	d.	£	s.	d.
Salaries:—						
Editor of Publications	60	0	0			
Assistant Secretary	130	0	0			
Commission on Collecting	33	16	0			
				223	16	0
Taxes:—						
Land and Assessed	6	11	0			
Income	2	1	8			
Poor Rate	4	15	10			
Other Parish Rates	2	18	4			
				16	6	10
Bills:—						
Strangeways and Walden, printers ...	173	4	0			
Rumfitt, bookbinder	30	3	2			
Basire, engraver	16	7	6			
Malby, lithographer	5	5	0			
Harrild, ,, ...	3	10	0			
Cooke and Sons, instruments	13	13	0			
Insurance	10	0	6			
				252	3	2
Miscellaneous items:—						
House expenses	26	8	3			
Postages	46	8	11			
Books and parcels	7	11	10			
Expenses of evening meetings	13	13	0			
Waiters attending meetings	3	17	0			
Coals and wood	12	0	0			
Gas	6	10	0			
Repairs	1	9	8			
Sundries	9	6	0			
				127	4	8
				619	10	8
Turnor Fund	1	13	6			
Mrs. Jackson's annuity, 1 year	8	15	6			
				10	9	0
Investment:—						
Purchase of £200 Consols...				188	5	0
Banker's Commission on Country Cheque				0	0	6
				818	5	2
Balance at Banker's				390	14	3
				1208	19	5
Add error of 4*d.* in the Petty Cash Account, carried to Petty Cash Account for 1869				0	0	4
				£1208	19	9

F. C. PENROSE,
H. PERIGAL,
J. H. DALLMEYER,
} *Auditors.*

Assets and Present Property of the Society, January 1, 1869:—

	£	s.	d.	£	s.	d.
Balance at Banker's	...		...	390	14	3
2 Contributions of 7 years' standing ...	29	8	0			
9 ,, 5 ,, ...	94	10	0			
16 ,, 4 ,, ...	134	8	0			
10 ,, 3 ,, ...	63	0	0			
23 ,, 2 ,, ...	96	12	0			
36 ,, 1 ,, ...	75	12	0			
Balance of an Account...	5	10	0			
2 admission-fees, and first contributions ...	6	6	0			
				505	6	0
Due for Publications				2	14	6
£5000 New 3 Per Cents (including Mrs. Jackson's Gift, £300).						
£3000 Consols, including the Lee Fund (£100) and Turnor Fund (£500).						
Unsold Publications of the Society.						
Various astronomical instruments, books, prints, &c.						
Balance of Turnor Fund (included in Treasurer's Account)				125	18	0

Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	13	XI.	188	XXIV.	189
I. Part 2	53	XII.	194	XXV.	199
II. Part 1	71	XIII.	205	XXVI.	206
II. Part 2	35	XIV.	394	XXVII.	460
III. Part 1	88	XV.	177	XXVIII.	421
III. Part 2	107	XVI.	203	XXIX.	452
IV. Part 1	107	XVII.	185	XXX.	201
IV. Part 2	118	XVIII.	181	XXXI.	177
V.	128	XIX.	191	XXXII.	208
VI.	154	XX.	186	XXXIII.	215
VII.	178	XXI. Part 1	216	XXXIV.	213
VIII.	164	XXI. Part 2	100	XXXV.	185
IX.	167	XXI. (together).	94	XXXVI. (with M. N.)	267
X.	179	XXII.	185	XXXVI. (without)	68
		XXIII.	182		

The instruments belonging to the Society are as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatorial,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock, No. 1,
The *Beaufoy* clock, No. 2,
The *Wollaston* telescope,
The *Lee* circle,
The *Sharpe* reflecting circle,
The *Brisbane* circle,
The *Short* universal instrument.

The *Sheepshanks'* collection of instruments, viz.,—

1. 30-inch transit, by Simms, with level and two iron stands.

2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

3. $4\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; double-image micrometer; object-glass micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters, with equatorial stand and clock movement.

4. $3\frac{1}{4}$-inch achromatic telescope, with equatorial stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

5. $2\frac{1}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

6. $2\frac{1}{4}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.

7. 2-foot navy telescope.

8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.

9. Repeating theodolite, by Ertel, with folding tripod stand.

10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.

11. Portable zenith instrument, with detached micrometer and eyepiece.

12. 18-inch Borda's repeating circle, by Troughton.

13. 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms.

14. A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff; in which the telescope tube is packed; repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.

15. Level collimator, plain diaphragm.

16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.

17. Hassler's reflecting circle, by Troughton, with counterpoise stand.

18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.

19. 5-inch reflecting circle, by Lenoir

20. Reflecting circle, by Jecker, of Paris.

21. Box sextant and 3-inch plane artificial horizon.

22. Prismatic compass.

23. Mountain barometer.

24. Prismatic compass.

25. 5-inch compass.

26. Dipping needle.

27. Intensity needle.

28. Ditto ditto.

29. Box of magnetic apparatus.

30. Hassler's reflecting circle, with artificial horizon roof.

31. Box sextant and $2\frac{1}{4}$-inch glass plane artificial horizon.

32. Plane speculum artificial horizon and stand.

33. $2\frac{1}{2}$-inch circular level horizon, by Dollond.

34. Artificial horizon roof and trough.

35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.

36. A pentagraph.

37. A noddy.

38. A small Galilean telescope, with the object lens of rock-crystal.

39. Six levels, various.

40. 18-inch celestial globe.

41. Varley stand for telescope.

42. Thermometer.

43. Telescope, with the object-glass of rock crystal.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Fuller* theodolite, to the Director of the Sydney Observatory.

The *Beaufoy* transit, to the Observatory, Kingston, Canada.

The *Sheepshanks* instrument, No. 1, to Mr. Lassell.
Ditto ditto No. 3, to Major Tennant.
Ditto ditto No. 4, to Rev. C. Lowndes.
Ditto ditto No. 5, to Mr. Birt.
Ditto ditto No. 6, to Rev. J. Cape.
Ditto ditto No. 8, to Rev. C. Pritchard.
Ditto ditto No. 9, to the Director of the Sydney Observatory.
Ditto ditto No. 41, to Rev. C. Pritchard.
Ditto ditto No. 43, to Mr. Huggins.
The 6-inch circular protractor, to Mr. Birt.

The Gold Medal.

The Council have awarded the Gold Medal to Mr. E. J. Stone for his Rediscussion of the Observations of the Transit of *Venus* in 1769, and his other contributions to Astronomy. The President will, in the usual way, explain the grounds of this award.

Memoirs.

The next volume of *Memoirs* will contain Major Tennant's paper on the Solar Eclipse of August 18, 1868. The paper will be accompanied by engravings of the photographs, enlarged under the superintendence of Mr. Warren De La Rue.

Index to the Publications of the Society.

An index to all the papers contained in the publications of the Society, from its commencement down to the end of 1868, is nearly finished, and will shortly be forwarded to press. The index will contain all the papers contributed by each author under his name, and also a list of papers contributed under the head of each subject.

Obituary.

The Society has to regret the loss by death of the following Fellows and Associates:—

Fellows.—Philip James Chabot.
Sir David Brewster.
Rev. W. R. Dawes.
Thomas Cooke.
H. E. Westcar.

Major-General Shortrede.
Robert Porrett.
W. S. Downes.
John Jenkins.
W. H. Barton.
Robert Worthington.

Honorary Fellow.—The King of Siam.

Associate.—Leon Foucault.

The late Rev. WILLIAM RUTTER DAWES, Fellow of this Society, was born on the 19th of March, 1799, at Christ's Hospital, where his father was Mathematical Master. He lost his mother while very young, and, his father having been appointed Governor of Sierra Leone, he was placed under the care of his grandfather at Portsmouth. In 1807 he was transferred to the family of the Rev. Thomas Scott, the revered Commentator on the Bible, where he remained till 1811, in which year, on his father's return to England, he was placed at the Charter House. In 1813, however, on his father's again leaving England for the West Indies, he was removed thence and once more placed under the care of the Rev. Mr. Scott, at Aston Sandford, where he remained until the death of that excellent man, who breathed his last in his pupil's arms.

Designed by his father for a clergyman in the Church of England, he studied under Mr. Scott with that object in view. A mind like his, however, could not receive dogmas, however consecrated by general approval, without giving them a searching examination. In his case the result was, that he became dissatisfied as to some at least of the tenets of the Established Church, specially relating to ecclesiastical government. Instead, however, of rushing headlong in the opposite direction, young Mr. Dawes turned his thoughts to the study of Medicine, for which he was eminently qualified by nature. Having gone through the usual course at St. Bartholomew's Hospital, under Abernethy and the late Sir Wm. Lawrence, he returned to the neighbourhood which in Mr. Scott's society he had learned to love, and settled as a medical practitioner at Haddenham, Berks. Here he married his first wife, the widow of his late tutor and friend, who, though much older than himself, contributed greatly to his happiness by her vivacity and Christian gentleness.

Domestic arrangements arising out of the death of an only and very dear sister caused him, however, to give up his practice at Haddenham, and brought him to Liverpool in the year 1826. Here his former desire to take holy orders seemed to revive; but he was unable to escape from the scruples which had formerly deterred him from so doing in the Church of England. While in this unsettled state of mind, Mr. Dawes was brought into contact with the late Dr. Raffles, Minister of the leading Independent Congregation in Liverpool. A man possessing such gentlemanly

feelings, and acting on such enlarged and Christian principles, at once exercised a powerful influence over Mr. Dawes, and he was at length induced to take charge of a small congregation of the Independent denomination at Ormskirk. The name of this place is the first which is associated with him in that scientific career which he afterwards adorned. Partial to Astronomy from a youth (for he has left sketches of his observations while with Mr. Scott — a taste inherited from his father) — Mr. Dawes continued his observations while in Liverpool with a small telescope (of less than 2 inches aperture) through an open window.*

At Ormskirk, however, he erected his first Observatory; on a small scale, the principal instrument being a 5-foot refractor by Dollond, aperture 3·8 inches; and there became known as an earnest and precise observer. On the 14th May, 1830, he was elected a Fellow of the Royal Astronomical Society, and, his health having some time previously failed, he was compelled to resign his ministerial charge, and in the early autumn of 1839† he took charge of Mr. Bishop's Observatory at South Villa, Regent's Park; himself residing in St. John's Wood. While here, it was that he married a second time (his former wife having died at Ormskirk some years previously). This happy and judicious union with the widow of John Welsby, Esq., Solicitor, of Ormskirk, which occurred in 1842, contributed materially to the comfort and usefulness of his future life. Soon after, on the termination of his connexion with Mr. Bishop in the spring of 1844, Mr. Dawes removed to Cranbrook, in Kent, to a residence called Camden Lodge, where he erected the Observatory described in the sixteenth volume of the *Transactions* of this Society, which, in 1846, he enriched with a fine equatoreal telescope of 8½ feet focus and 6 inches aperture, by Merz, of Munich, admirably adapted by a most delicate clock-work movement for his observation and measurement of double-stars, and the satellites of *Saturn* and *Uranus*. With this he continued indefatigably to observe, suffering, however, deplorably from continual and most distressing headaches and other symptoms of increasing ill-health, which, at length, obliged him to resort to a residence at Torquay, and had

* This was probably the telescope referred to in the following extract from one of his letters, dated 17 Dec. 1867, addressed to Sir J. Herschel: —

"Having obtained the loan of a volume of Rees's *Encyclopædia*, I had copied out the list it contained of Sir Wm. Herschel's Catalogues of Double-Stars, arranged in classes and constellations; and with a capital little refractor of only 1·6 inches aperture, and a copy of the French edition of Flamsteed's Atlas, which was presented by Dr. Maskelyne to my father before he went out in 1787 as Government Astronomer to the first expedition to Botany Bay under Governor Philip. I worked away on almost every fine night, when uncertain health would permit, and found and distinctly made out a large number of the objects in the second and third classes, among which were *Castor*, *Rigel*, ι^1 and ι^2 *Lyræ*, σ *Orionis*, ζ *Aquarii*, and many others, of which I made correct diagrams in a book now lying before me, as well as of γ *Virginis* (then in the same quadrant that it is now), *Polaris*, &c. &c. The difficulty often was to get to bed in summer before the Sun extinguished the sight of the game."

† Some time between Aug. 10 and Oct. 1.

nearly decided him to dispose of his instruments and give up observing. A favourable change, however, took place, and in 1850 he removed his observatory to Wateringbury, near Maidstone, and finally, in 1857, to Hopefield, Haddenham, near Thame, where he continued to reside till his decease. Here it was that the accurate observations and ripened experience of many years bore their fruit in the universal esteem with which they were regarded, as well as in the volume completed and printed just before his death by the Astronomical Society, under the title of "Catalogue of Micrometrical Measures of Double Stars," forming part of the thirty-fifth volume of their *Transactions*. This work, taken in conjunction with the earlier series of measures of 121 double-stars, contained in vol. viii. of the *Mem. R. Ast. Soc.*, and of his Ormskirk Measures, communicated to the Astronomical Society in 1849–50, and forming part of the nineteenth volume of their *Memoirs*, and with those made with Mr. Bishop's 7-inch refractor from 1839 to 1844, published by that gentleman in a separate volume of observations at South Villa, form a monument of unwearied industry and minute and painstaking precision, to which it would be difficult to find a parallel in the record of private and unassisted observation. The instrumental means at his disposal in the interval from 1854 to 1865 were augmented by the purchase in 1854 of an admirable 7½-inch refractor by Alvan Clarke, which was replaced in May 1859 by a still larger instrument by the same maker of 8¼ inches clear aperture and 110 inches focus. equatoreally mounted, at Haddenham, as described in the *Ast. Soc. Notices*, vol. xx. p. 60. Many of his measures, too, commencing with April 13, 1865, were made with an 8-inch refractor by Messrs. Cooke and Son, so that nothing which instrumental means could supply is wanting to render these later observations landmarks of ultimate and invaluable reference in this department of Astronomy.

In the year 1855 Mr. Dawes was presented with the Gold Medal of the Royal Astronomical Society. In his address to the Society on that occasion, the Astronomer Royal (the President for the year), while laying all due stress on the excellence of Mr. Dawes's measurements of double-stars from 1831 to 1844, very properly calls attention to his *independent discovery* in (Nov. 25 and 29) 1850 of the interior dusky ring of *Saturn*, noticed by Prof. Bond on the other side of the Atlantic, on Nov. 15th of the same year, of which observation the first account reached England on Dec. 3d. The astonishing phenomena of the total Solar Eclipse of 1851, too, as he states in the same address, were observed by Mr. Dawes, who made the voyage to Sweden for that purpose.

In 1865 Mr. Dawes was elected a Fellow of the Royal Society.

Endowed with great perseverance and schooled to great accuracy of observation and description, Mr. Dawes's contributions to astronomical science will ever be regarded as of high value.

He possessed also great ingenuity, and was constantly experimenting. His solar eye-piece, which, by stopping out the blinding light of the photosphere, allows us to obtain a distinct and prolonged view of the interior or nuclear portion of the spots, has afforded us as its practical result, the knowledge of the existence of an interior envelope of the Sun's body which had never before been suspected.

Devoted as he was to Astronomy, the subject of this brief notice was ever ready to impart gratuitous advice to the sick, and will be remembered as their benefactor for years to come by the families of the poor inhabitants of Haddenham; and while his name is known and revered among the learned of different nations and remembered with affection by his relations and friends, it will ever be cherished with grateful feelings by the many recipients of his seasonable medical advice and treatment.

After the loss of his second wife, which occurred in December 1860, his health, always feeble, as a natural consequence of the deep depression of his spirits, gradually gave way. In addition to his habitual and distressing headaches, and attacks of nervous and spasmodic asthma, a heart affection manifested itself, rendering perfect stillness for hours together, and avoidance of the slightest excitement, necessary.

In the intervals of comparative ease accorded him from these attacks, however, he was able to continue his observations, the results of which up to the middle of 1865 (so far as the measurements of Double Stars are concerned)are given in an Appendix to the great Catalogue already spoken of. Much of his attention, too, was given to the discussion as to the nature of that peculiar mottling of the solar photosphere which has been compared to "willow-leaves" and to "rice-grains," but in which he could never be brought to allow that kind of generic form and magnitude which would go to stamp them as "entities" of some special kind modelled according to some definite type: likening them (while not denying their occasional and even frequent visibility as bodies having *size* and distinguishable *shape*) rather to bits of straw or irregular flocculi; nor can this persistent testimony, coming from an observer of such singular acuteness of vision and of such unwearied patience in scrutiny, be lightly cast aside.

Two years before his death Mr. Dawes had a serious attack of illness, from which he so far rallied that his friends hoped his valuable life might yet have been spared many years. Some weeks before his last attack he had been ailing, and seemed prepared for something serious. Up to the last he retained the entire possession of his faculties and his interest in his astronomical pursuits. Only twenty-four hours previous to his decease he had his "Measurements" on his knee in bed, explaining something there to his medical attendant.

Combined with an ardent, inquiring, and mathematically reasoning mind, Mr. Dawes possessed an implicit and childlike faith in the great doctrines of Revelation, and, though ready to dispute

many of the accepted theories of theology, never for a moment let go the cardinal doctrine of salvation by faith only in our Lord Jesus Christ. After some hours of unconsciousness, he peacefully breathed his last on Feb. 15, 1868, and lies interred beside his dear wife in the churchyard of Haddenham.*

The late Major-General ROBERT SHORTREDE was born at Jedburgh, in Roxburghshire, on the 19th July, 1800, and was the son of Mr. Robert Shortrede, Procurator-Fiscal of that county. He was educated first at the Jedburgh Grammar School, and afterwards at the Edinburgh High School, whence he went to Edinburgh University, where he attended the Mathematical and Natural Philosophy classes for four years, and attained considerable distinction. During that time he was also in the office of Mr. Robert Stevenson, the engineer of the Northern Lighthouses, as he was originally intended for a civil engineer, having early evinced unusual aptitude for Mathematics; but his friends, thinking that India presented ample scope for his talents in that direction, he obtained an appointment to that country from the late Mr. Canning, through the influence of Sir Walter Scott, an old and intimate friend of his family; but unfortunately he was then too old to go to Addiscombe, and so into the Engineers in the usual course, and was therefore obliged to enter the Infantry of the line, which he afterwards found to be a serious hindrance to his scientific advancement. Shortly after his arrival in India in 1822, he was appointed to the Deccan Survey, and was soon afterwards employed to conduct a triangulation over the Bombay Presidency, in the course of which he measured a base-line on the Karleh Plain, which when connected with that from Seronj by the Bombay Longitudinal Series, has proved to be not inferior in correctness to bases measured in the Great Arc. In 1834 at the request of Lord Clare's government, he was appointed to examine and report upon an extensive Revenue Survey and Assessment of the Deccan, in the course of which it was believed that great frauds had been committed. For the manner in which this duty was performed he received the thanks of Government, and shortly afterwards, without solicitation on his part, was put in civil charge of a district which had long been in disorder, with almost discretionary power, to revise the rates, and put it into order generally. He lowered the assessment and increased the revenue, and in the course of two years brought 40,000 acres of land into cultivation on moderately increasing leases. While so employed he had a dangerous illness, and before the end of his sick-leave, on the sudden death of the Mint Engineer, he was employed for a few months as assistant in the Mint until some permanent arrangement could be made. Shortly afterwards Captain Jacob, then Astronomer at Madras, went on sick-leave to the Cape, and Captain Shortrede was put in charge of the work t'

* The leading biographical features of this notice were communicated and are given in great measure in the words of, a near relative of the deceased

his return. He was afterwards permanently appointed to the Great Trigonometrical Survey, in which he remained till 1845, when he came home home on sick-leave, and while in England drew out a paper on the attraction of the Himalayas as affecting the northern end of the Great Arc, which was shown to and approved of by Lieutenant Raper, Admiral Smyth, Sir J. Lubbock, and Sir J. Herschel, and afterwards submitted to the Royal Astronomical Society, and printed in their *Transactions* for 1845. He also published at this time some Mathematical and Logarithmic Tables, probably the most extensive and correct ever published by a private individual without a subscription list, and which have received the approval of several distinguished mathematicians and astronomers, both British and foreign, amongst others the Astronomer Royal. In 1849, he returned to India, and was appointed to the Revenue Survey in the Punjaub, where he remained till 1856, when he was ordered with his regiment (the Second Bombay Europeans) to Persia, and served through that campaign: he returned to England in 1859. He soon afterwards retired, since which time he published some Traverse Tables, and was engaged at the time of his death in correcting the proofs of some Navigation Tables, compiled with a view to facilitate keeping a reckoning at sea. Throughout his career he was an earnest Christian and philanthropist. He died at Blackheath after a few hours' illness from disease of the heart, on the 26th November, 1868, aged 68.

Mr. John Jenkins was born in London in 1802; while young he accompanied his parents to Swansea, where he subsequently carried on the business of a watch and chronometer-maker,—a vocation which seems to have had considerable influence in determining his pursuits in after years. His contributions to local scientific observation commenced at an early period of his life. His meteorological observations were conducted during a long series of years, and the voluminous tables supplied by him on occasions of more than one official inquiry, attest his unwearied industry, while they obtained for him the admiration and esteem of eminent scientific men engaged in those inquiries. In 1842, Mr. Jenkins accomplished his long-cherished wish of erecting an observatory on his business premises in Swansea. In effecting this object he consulted not only his own tastes for astronomical observation, but the convenience of the numerous foreign trading vessels which about this time began to resort to the port; the observatory was open to all masters and other officers of ships trading to Swansea. It may be remarked that the transit instrument in this observatory was fitted up under the personal superintendence of the late Mr. Sheepshanks, for whom Mr. Jenkins entertained the highest regard, and through whose instrumentality he became a fellow of the Royal Astronomical Society. Here after the business engagements of the day, Mr. Jenkins was for many years in the habit of devoting the hours of night to astronomical observation. In all local efforts for the promotion of the

interests of science and the diffusion of useful knowledge Mr. Jenkins took an active and often a prominent part. His labours on behalf of the Royal Institution of South Wales were for many years unremitting, and were duly acknowledged by that body on his retirement from official connexion with it some two years since. Referring to organisations of a more popular character for the diffusion of scientific knowledge, so long ago as 1826 we find Mr. Jenkins a prominent promoter of the first attempt to establish a Mechanics' Institution in the town of Swansea, and in all subsequent efforts for the same object he was equally prompt in giving personal services and pecuniary aid to the cause of popular enlightenment, while his sympathies with the social and moral amelioration of the industrial classes are attested by the fact recorded in a local notice of his death that "his patient labours over many years in the promotion of Benefit Building Societies in Swansea and in its neighbourhood give him signal claims to public remembrance." Mr. Jenkins retired from the business which he had so successfully conducted some fifteen years since. For the past three years he had been in declining health from a disease, the sufferings of which he bore with fortitude and equanimity, but under which he finally sank, leaving behind him the cherished recollection of one who, according to his opportunities, had well discharged the "stewardship of life." He died at his residence, Rotherslade, Swansea, on December 16, 1868.

Mr. William Samuel Downes was the eldest son of Mr. James John Downes, whose death was recorded in last year's Report. He evinced in early life great literary and scientific ability, but receiving at the early age of twenty-seven the important and responsible appointment of actuary and secretary to the Law Life Assurance Society, he thenceforth devoted his whole mind to the conduct of the affairs of that important institution. He was one of the founders of the Actuaries' Club, and acted as its treasurer for some time. He died on the 5th of December, 1868, in the forty-ninth year of his age.

Robert Porrett was born in 1782. His father held the office of Ordnance Store-keeper in the Tower, to which office the subject of the present memoir succeeded at his decease. He became a member of the Spitalfields Mathematical Society in 1811. He was an experienced chemist. His first communication in that science was printed in the *Transactions of the Society of Arts*, vol. xxvii. 1809, it is entitled "On Prussic and Prussous Acids." In 1813, in conjunction with Messrs. Wilson and Rupert Kirk, he published an important series of experiments on the explosive compound of chlorine and nitrogen, in vol. xxxiv. of Nicholson's *Journal*. The *Phil. Trans.* for 1814 contain a paper of his "On Triple Prussiates and on Acids formed by the Element of Prussic Acid with Sulphur," and another entitled "On the Constitution of the Prussic, Ferro-chyasic, and Sulpho-chyasic Acids." He gave the name of ferro-chyasic acid to that acid chemists

now call hydrogen ferro-cyanide, from its being compounded of iron, carbon, hydrogen, and nitrogen. His investigations on endosmose or the elective transmission of liquids through a membranous diaphragm was published in the *Annals of Philosophy*, vol. viii. 1816. This curious property of membranes has since been thoroughly investigated by Prof. Graham, whose researches have ultimately led to a new method of analysis called "dialysis." He contributed many valuable essays to the other scientific journals, the latest in conjunction with the late Mr. Teschmacher was read before the Chemical Society in 1846, it is entitled, "On the Composition of Gun-cotton." Throughout his career as a chemist he was distinguished for the great originality of his views. He was elected a Fellow of the Royal Society in 1848, and was also a Fellow of the Society of Antiquaries and of the Chemical Society. He became a Fellow of this Society on its junction with the Mathematical Society in 1845. In addition to his chemical pursuits he was much devoted to antiquarian researches, particularly as connected with ancient weapons and armour, and was always ready to give any information in his power on such subjects. He was for many years Treasurer of the Chemical Society, and was on more than one occasion pressed to accept the Presidentship, which he declined on the ground of his advanced age, which, however, did not prevent his being a frequent attendant at the meetings of the various scientific Societies with which he was connected. He died on the 25th of November, 1868.

Philip James Chabot was born in Spitalfields in 1801. His father, who was descended from one of the refugees who were obliged to leave their native country in consequence of the Revocation of the Edict of Nantes, was an eminent dyer in that parish. After the usual classical education, Mr. Chabot entered at St. John's College, Cambridge, and took his degree of M.A. in 1827. In 1830 he was called to the Bar, but, on the decease of his father in 1832, he was induced by the earnest entreaties of his friends, although much against his own inclination, to quit the legal profession and engage in his late father's business as a dyer. He subsequently made several improvements in some of the branches of dyeing; but finding the business did not answer his expectations, he retired from it, and became the originator of the Silk Conditioning Society, of which he was Secretary and Manager to the time of his death. He was elected a Member of the Spitalfields Mathematical Society in 1834, and at the junction of that Society with this in 1845, he became a Fellow of this Society. He was a Member of the Cavendish and Philological Societies, and also of the Chemical Society, from which he had retired a few years before his death. He was a well-read and intelligent man, and much interested in science and general literature, and was greatly respected for his probity and urbanity of manners. He died suddenly January 11, 1868.

William Parsons, Earl of Rosse, the eldest son of Lawrence,

the second Earl, was born at York on the 17th of June, 1800. His mother was Alice, daughter of John Lloyd, Esq., of Gloster, King's County. His preparatory education he received at home, from a private tutor, and in 1818 he entered the University of Dublin; remaining there, however, only till the following year, when he passed to Magdalen College, Oxford. Here he took high honours, receiving his B.A. degree as a First Class in Mathematics, in 1822. While bearing the title of Lord Oxmantown he was elected to represent King's County, and sat in Parliament from 1821 till 1834, when he retired from political life — for a while only, however — to devote himself more closely to the duties of his social position, and to follow with greater freedom the philosophical pursuits into which the bent of his mind had led him. In the meanwhile, in 1831, he had been appointed Lord Lieutenant of his County, and in 1834 he was made Colonel of its Militia. Two years after this he was married to Mary, eldest daughter of the late John Wilmer Field, Esq. of Heaton Hall, Yorkshire: in 1840 the present Earl, the eldest of four sons, was born.

The second Earl died in the year 1841, and the subject of this memoir succeeded to the title; but his Irish coronet giving him no seat in the House of Lords, he was still enabled to pursue without serious interruption the objects of his scientific taste. In 1845, however, he was elected an Irish Representative Peer, and thus he held a seat for life in the House of Lords. Although he occupied it with good regularity, he took little active part in debate: his voice was seldom, if ever, heard during the stormy period which embraced the discussions upon Irish Emancipation and Reform. But if not a talking, he was a working member, an invaluable referee in matters of business, and an assiduous servant upon committees. Throughout his Parliamentary career he gave his support to the Conservative party. Dwelling among an excited people in troublous times, he was firm in enforcing the authority of the law, even to the risk of his own personal safety; but withal he was kind and beneficent, devoting nearly all the rental of his Irish property to the relief of his suffering countrymen, thereby winning for himself a place in their hearts, and in time diverting the hostile feelings with which he had been regarded. His genius was eminently versatile. One among his friends, who had the best opportunities for discerning the extent and varied nature of his abilities, tells us that few minds of our day have grasped so wide a range of knowledge. Of his engineering skill and his mechanical resource he has left us monuments that speak for themselves; but of his mastery of political economy, of his capacity as a chemist, of his acquaintance with military and nautical sciences, he has left us no such memorials, though it was quite in his power to have created them.

Astronomy was the science of his adoption; the manufacture of stupendous telescopes the branch of it to which he devoted undivided attention. It was his aim to extend this to its farthest limit of practicability; to make a telescope of the largest dimensions possible with the resources of his time. The reflecting

principle was that to which he was compelled to resort. There was no hope of any such immediate advance in the manufacture of optical glass as would supply disks of the size and quality requisite to surpass to any degree the refractors then in use. We are speaking of the date of his first turning attention to the subject, which was about 1826. He made some few experiments upon fluid lenses, but these gave no promise. To perfect the reflector was the only course open to him. But it was one beset with difficulties that could be foreseen, and obstacles that arose only as the work grew under his hand. To start with, the Earl of Rosse had to originate everything; the experiences of his predecessors in the same domain of art were of no avail to him. Short, who had excelled all who went before him in casting and polishing specula, was so jealous of posterity reaping the fruits of his knowledge, that before his death he ordered all his tools to be destroyed. The Earl of Stanhope made great endeavours to attain magnitude in reflecting telescopes, but his attempts were failures. He bequeathed his papers to the Royal Institution, and Sir James South searched them through for memoranda bearing upon the subject, thinking that the failures of such a man might in their way be instructive; but he did not light on a single hint that was worth remembering. Sir William Herschel was the master who, from the majestic scale of his instruments, might have been expected to have given the Earl of Rosse an example to work from; but his methods were a sealed book—what was the composition of his metals and how he worked them to figure and polish have never been made known.

The inaugural difficulty was in the selection of an alloy possessing the qualities of whiteness, brilliancy, and resistance to tarnish requisite for a mirror. After many trials a mixture was decided upon as answering these conditions,—an alloy of four atoms of copper to one of tin. But such a compound, though harder than steel, is more brittle than glass, very unmanageable in the founder's hands, not yielding a sound casting by ordinary processes, and when cast liable to crack in cooling, or afterwards by any sudden change of temperature. To form a large speculum in one mass of such material seemed impossible, and Lord Rosse was led to the expedient of building one up of small segments, separately cast and polished, and united upon a foundation-plate formed of a brass specially prepared that its temperature expansion should be identical with that of the speculum metal. Repeated trials upon a small scale were made, and at length a mirror of 3-feet diameter was completed, which performed well upon small stars, but that exhibited its divisions upon the disk of a large star. In the course, however, of the foundry experiments upon plates from which to cut the parts of composite specula, Lord Rosse was led to a method of casting sound slabs of metal of large size at one operation. The points to be secured were, cooling without cracking, and freedom from pores or air-bubbles. The ordinary sand-mould allowed the edges of the

plates to solidify first, and the central portions were confined and strained when their moment of cooling came, and this was the cause of cracking. It occurred to Lord Rosse that if the metal could be cooled from its lower surface upwards, in laminæ or strata, as it were, the upper surface being the last to solidify, this evil would be remedied. He tried an iron mould and threw a jet of water upon its under surface when the molten metal was within. This answered the immediate end, but the iron cracked and spoilt the casting. The next idea was to make the mould with a stout iron bottom and sand circumference, and trust to the abstraction of heat by the iron alone to cool the metal from below. This succeeded; but another evil betrayed itself: the air entangled between the speculum metal and the iron, formed large cavities that ruined the disk; in the porous sand-moulds it found a ready exit. The removal of this defect was the culminating triumph in this department of the noble founder's labours. Equal to every emergency he proceeded to make a ventilating bottom for his mould: he took thin bands of iron and closely packed them edgewise into a frame the size of the intended speculum, so that instead of a solid plate he had a surface somewhat like a book-edge; close enough to prevent the viscous metal from leaking out, but porous enough to allow the air to escape freely. The face of such a disk, turned in a lathe to the desired convexity and made smooth, formed the base of a mould, which, with a sand ring for its sides, permitted the casting of sixteen slabs for 3-feet specula, of which not one was defective. This improvement removed the last barrier to the casting of mirrors of any required diameter.

After meeting and surmounting more difficulties connected with the manufacture of crucibles, and after the exercise of anxious patience during the fortnight occupied by the cooling of the metal in an annealing oven, Lord Rosse obtained a 3-foot disk without fault or blemish, ready for the operations of grinding and polishing. It had been thought that the hand of a workman was the only fit guide for the polishing tool of a reflector, that the process was one which must be *felt* to be successfully followed. Such a notion was a manifest impediment to attempt, even in the manuafacture of large telescopes. Lord Rosse was compelled at an early stage of his experiments to resort to machine power. The manual operation of figuring a speculum is pretty well known: the tool is mounted upon a pedestal, and the workman, grasping the metal disk, gives it a rectilinear or curvilinear motion in a direction perpendicular to the plane of his body, at the same time walking round the pedestal. To reproduce these combined motions, with the means of methodically altering them and maintaining them in their altered condition, was the object of the polishing apparatus invented by Lord Rosse in 1828, and which was employed, with small modifications, in the many hundred grindings and polishings which he caused to be performe

he drawing and description of the machine are to be found

the *Philosophical Transactions* for 1840, and elsewhere; it is not necessary that we should describe it further than by saying that while the speculum rotates horizontally in a bath of water, for preservation of a uniform temperature, the polishing tool, weighted as may be necessary, lies upon it, and moves either in a straight line or in an ellipse the relation of whose axis is varied by adjustible eccentrics carrying the rod to which the tool is attached. With this mechanism every motion could be defined, and its effect upon the speculum's figure ascertained; a particular result once obtained could be repeated at will; and a precision in the figuring of reflectors was secured which, till this system was introduced, had been out of reach.

All difficulties of grinding overcome, and the figure tested by the image of a watch dial carried on a mast fifty feet above the polishing machine—so that the trials could be made without moving the speculum—new obstacles arose in the process of polishing. The grinding tool was covered with pitch to form the polisher, but the unequal expansion of the bituminous layer, by pressure and rubbing, destroyed its form, so that it would no longer fit the speculum, but chafed it untruly. This difficulty, never felt in working small mirrors, because the pitch layer can expand equally where its area is small, was a puzzling one to Lord Rosse working upon his large scale. But he overcame it at last, by scoring the resinous surface of the tool with grooves, and subsequently, as even this plan sometimes failed, by furrowing the iron tool itself. So long as the pitch was soft enough to escape into the grooves, all went well, but this softness was an impediment to the production of a true figure, which required that the medium for holding the particles of polishing powder should be as hard as possible. It takes a short time to relate what took a long time to invent. Two opposite properties, hardness and softness, were united in the resinous coating: first a layer of soft resin—which Lord Rosse eventually preferred to pitch—was applied, and then a layer of hard; the first acted as a cushion to the second, and thus the requisite conditions were realised. This was the final remedy of the final difficulty in the way of constructing great reflectors.

Such is an imperfect outline of the major obstacles that the Earl of Rosse encountered and overthrew during the twelve years ending with 1840, the date at which the 3-foot speculum was finally perfected. Of the minor embarrassments and perplexities we know nothing; but they must have been numberless. Throughout his original memoir are scattered modest sentences such as "after many difficulties" or "as the result of repeated trials," which must cover cares and anxieties beyond the appreciation of those who have not in some degree experienced similar troubles. The perfection of a great invention or a great instrument is rarely the work of a single labourer, but to the honour of Lord Rosse be it recorded that, unaided, he perfected the reflecting telescope. The 3-foot mirror, supported and mounted, surpassed, so far as

comparisons could be inferred, the definition and power of the larger telescope of Sir William Herschel, and this was the only predecessor that could be considered to compete with it.

Yet after all it was but the stepping-stone to higher ends. Lord Rosse now saw no impossibility in doubling the dimensions he had already attained, and he immediately determined upon attempting, or rather achieving—for with him the words were synonymous—a 6-foot speculum. Undeterred by predictions of failure, he set about the task. The leap from a mirror weighing a ton and a quarter to one whose weight would be four tons and a half involved new contrivances and precautions. But so thoroughly had Lord Rosse mastered every point of the process that every difficulty could be foreseen and provided for. This was done so completely that no trying back was necessary, no single operation or piece of work had to be repeated. The principles of forming the mirror were perfectly similar to those worked out for the 3-foot; only enlargement of means were necessary. The crucibles and melting furnaces had to be multiplied, and an annealing oven specially built to receive the stupendous casting; and the grinding and polishing machine had to be adapted to the larger size. The mounting of the instrument necessarily differed considerably from that of the 3-foot, which was similar to the older instruments of Herschel and Ramage. It was evident that the pyramidal frame of ladders which gave stability in their case would fail upon the scale of their giant successor. So it was resolved to sacrifice a large part of the sky view, and mount the instrument between two walls or piers, thus limiting its range to half an hour on each side of the meridian, at the equator, but giving it range in altitude from near the south horizon to below the pole. It may be that in this restriction we have an indication of the approach to impracticable magnitude, although Lord Rosse affirmed that a larger reflector than this could be made and would be of service, and held that where observations are systematically carried on the confinement in Right Ascension was no great disadvantage.

The great speculum was cast on the 13th of April, 1842, the eve, by the way, of his lordship's wedding anniversary, and was completely mounted for use by February, 1845, when Dr. Robinson and Sir James South visited it to make some inaugural observations. The latter observer gave his impressions in a lengthy letter to the *Times* of April 16 of that year. His comments are full of enthusiasm and admiration for the sights which he beheld, and which he avowed had never been gazed upon by man before. Dr. Robinson's experiences were given verbally to the Royal Irish Academy almost upon the same day. We need not dwell upon his remarks, interesting and instructive as they are, for in the descriptions of the instrument's work during the subsequent years of its use we have a wider view of its capabilities. We have seen how it has with certainty resolved certain of the nebulæ, and how, for a while, till spectrum analysis arose to arrest the conclusions it

tempted, it threatened to disprove the existence of such things as nebulæ at all by bringing all the objects so called within the domain of star-clusters; and we have seen how it has in this same class of bodies revealed a structure and arrangement more wonderful and inexplicable than anything which had hitherto been known to exist—we allude to the spiral conformation in all its varying conditions; we have seen, too, how it has necessitated some modifications in our previous ideas regarding planetary nebulæ, by breaking up the symmetric outlines that gave them their distinctive name. Lastly, we have seen, in the memoir recently published, that the views it has given of the nebula in *Orion* at intervals of fifteen years, as recorded by trustworthy draughtsmen, betray strong indications of change in the disposition of certain parts of the nebulous matter. On this point, as on many others, the instrument may yet yield results surpassing anything yet reaped from it; indeed its work, presided over by a new master inheriting the astronomical tastes of its founder, may be said to have but just begun. Already the spectroscope has been applied to it, and a few steps have been taken into that field of research for which, from its light-grasping power, it is so peculiarly fitted—the analysis of the light of the fainter stars and nebulæ. To pursue this a clock-movement is necessary; one is being, or by this time has been applied; so that ere long we may reap a harvest of knowledge upon a subject that was unknown when the instrument was designed, and thus will be added new honour to the fame of its designer.

The scientific life of the Earl of Rosse is the history of the leviathan telescopes. Their achievements are his renown. Posterity will esteem him rather as an engineering than as an observing astronomer. The honour that in the department of mechanical science we should accord to the great machine-tool makers who have rendered our age famous belongs in the department of astronomy to him. There is no disparagement implied in this comparison: far otherwise. It may justly be urged that the maker is above the user of an implement. Eyes are common to us all, all could make discoveries if they had the means. It was the means that the Earl of Rosse supplied. In his instruments we have the fruits of varied resources that are seldom combined in a single individual; great mechanical ability, optical and general scientific knowledge, and a large fortune; to these may be added indomitable perseverance and an imperturbable temper. Without these last the works he projected must have broken down. Be it remembered that he had no skilled workmen to help him, but drew all his personal assistance from the labourers about his estate, and he had to educate every man to the work. Everything was done in his own workshops and laboratory, even his steam-engine was of home construction, and at one time he was compelled to cast his own crucibles for the founding of his specula. But for the happy union of tastes, abilities, and means, such an instrument as the 6-foot reflector could not, in its time at least, have been

brought into existence; the undertaking would have been beyond the powers of professional instrument-makers, and beyond the purse of any individual or body likely to be interested in its prosecution. Since, however, its maker proved the possibility of producing perfect mirrors of immense size by mechanical means, others, profiting by his experience and advice, fully published and freely imparted, have excelled in the work, and now a 4-foot reflector, constructed by Mr. Grubb, under the superintendence of a committee, of whom Lord Rosse was one, has been sent to Australia to re-explore the depths of the Southern skies. Lord Rosse's freedom in making his methods accessible to the world will be remembered with gratitude by those who may follow him in the same field of labour; and by those of us who have no such object it must not be overlooked, for it shows us that his aim was not the vainglorious possession of something beyond the reach of his fellow-men—in which case he would have preserved silence—but the extension of Astronomical research in his own and succeeding generations.

His Lordship joined this Society in 1824, and the Royal Society in 1831; he was President of the latter from 1849 to 1854, and received its Royal Medal, for his telescopic triumphs, in 1851. He presided over the British Association Meeting at Cork in 1843; and he was a Visitor of the Maynooth College and a Member of the Visiting Board of the Greenwich Observatory. During the last six years of his life he filled the post of Chancellor of Trinity College, Dublin. He died at his seat and the scene of his life's labours, Birr Castle, Parsonstown, King's County, on the 31st of October, 1867, from the effects of a surgical operation—the removal of a tumour produced by a slight sprain of the knee. His remains were deposited in the Church of St. Brandon, Parsonstown, having been followed to their resting-place by some 4000 of the tenantry on his estates, by whom he had been deservedly beloved. [J. C.]

Thomas Cooke, by the combined force of his character and his genius, raised himself from a very humble sphere in an obscure village to a foremost place among that long line of eminent opticians who have contributed in no small degree to sustain the high reputation of England for practical science.

He was born on the 8th of March, 1807, at Allerthorpe, in the East Riding of Yorkshire; his father being a shoemaker in that village. So straitened were the circumstances of his home, that, although the boy in many ways showed tokens of no common abilities, nevertheless his father could afford him no better education than that provided by the National School, and even this instruction was limited to the space of two years. For this reason he was, at an early age, put to his father's trade, but for this trade young Cooke appears from the first to have felt an unconquerable aversion. The boy was for ever thinking o

Astronomy rather than of the mending of shoes. In those days, some of us can well remember how the voyages of Captain Cook exercised a wonderful fascination over young minds, and even now they might with great advantage displace much of the current literature of the times: over young Cooke's mind the fascination was irresistible. His bent was for the sea, and he would become, he said, a second Captain Cooke. But even here that common sense and practical side of the boy's character presented itself, which at later periods of his life became so salient a part of the character of the man. Thus, in selecting for himself the life of a sailor, he was so far from being a visionary, that he set himself at once to learn navigation, in order that he might make himself a good seaman. When, however, all was prepared for the final step, and young Cooke was on the point of starting on his new career, the entreaties and the tears of his mother prevailed on his feelings, naturally affectionate, and he set himself to consider in what way he could provide for his own maintenance nearer home. He renewed his studies with such small aids as he could procure, and in his seventeenth year he opened a village school. In this occupation he persevered until the age of twenty-two. This was the chief period when young Cooke was educating his own mind, and those who in after years had the pleasure of freely conversing with him on a variety of topics, must have been very sensible of the solid and successful nature of the result.

About the year 1829 he removed from Allerthorpe to York, where, for the next seven years he supported himself partly as an assistant-master in schools, and partly by private tuition. Such time as he could spare from this laborious and more remunerative occupation he devoted to mathematics and practical mechanics. In due course his thoughts were directed to the science of Optics, and, as was natural to one of his practical turn of mind, he attempted the construction of a reflecting telescope. Not satisfied with what he considered the uncertainty of the results of this form of instrument, and his pecuniary resources being unequal to the purchase of the requisite metals, he turned his attention to the manufacture of an achromatic object-glass. The material for one of the lenses he found in the bottom of an ordinary drinking glass; and although he had never seen the practical methods of grinding and polishing lenses, he at length succeeded, after many and wearisome trials, in producing a small achromatic telescope, the performance of which was sufficiently satisfactory to encourage him to further efforts. This telescope was subsequently purchased by Professor Phillips, of Oxford, who at that time was the centre of intellectual activity in York: no wonder that he was then, and continued to the last, the kind, warm friend of such a man as Cooke. For the counsel and encouragement afforded by that eminent *savant*, Mr. Cooke often expressed his gratitude in the hearing of the writer of this notice. Other friends also in York came forward, and held out the right hand of

fellowship to one in whom they discerned the evidences of no ordinary gifts.

With much hesitation and misgiving, Mr. Cooke now exchanged his avocation of a private teacher, the pecuniary returns of which, however inconsiderable, were in a measure certain, for the greater uncertainties of the business of an optician in York. Mr. William Gray, a Fellow of this Society, had the honour of giving him his first considerable order. It was for an equatoreally mounted telescope of 4½ inches aperture. Thirty years ago there were scarcely any telescopes of even that moderate aperture in this country, executed by English artists. It was not on account of any want of skill on the part of our native opticians, but mainly owing to unwise fiscal arrangements in relation to the excise on glass, that it was then customary to send for the few considerable telescopes required, to the successors of Frauenhofer at Munich. Hence it was nothing short of an important event at that time, for an English artist to undertake the construction of a large telescope, and still more so for a man, as yet unknown to fame, and living in a provincial town. Since that day matters in this respect have greatly improved, and at the present moment there is probably no astronomical or optical instrument which cannot be procured from one or another of our great native artists, on pecuniary terms quite as favourable, and of a quality at least as excellent, as any that can be procured from abroad. In effecting this result for the achromatic telescope, Mr. Cooke was a great pioneer.

Mr. Cooke appears to have considered that the success attending Mr. Gray's telescope laid the foundation of his fame as an optician. In 1851, an order was given by Mr. Pattinson of Gateshead, for an equatoreal instrument completely mounted in every respect, and of the then considerable aperture of 7 inches. This instrument, by its excellence, greatly contributed to the growing reputation of the artist. It was this telescope which was so munificently placed by its owner at the disposal of Professor Piazzi Smyth for the purposes of his well-known expedition to Teneriffe.

Orders now flowed to Mr. Cooke for specimens of his art, which more than tasked his limited means to supply. In consequence of this agreeable pressure, he purchased some land in Bishophill, York, and there erected those workshops which are now known as the Buckingham Works. Thither he carried his business in 1855, with but five or six workmen and one apprentice: at the present time they furnish employment for upwards of one hundred persons.

Mr. Cooke's reputation was thus established, and in the course of a very few years he constructed no less a number than nine complete equatoreals, having apertures varying from 8 to 10 inches. Some twelve or thirteen others were also constructed, varying from 5 to 8 inches in aperture, the smallest of which deserved the name of a noble instrument. It is a fact worthy

of remark that all these large telescopes were commissions given to him by *private individuals*, living in a country of so small area as our own.

In the successful manufacture of the telescope with its elaborate mounting, Mr. Cooke possessed the great advantage of being not alone an accomplished and theoretical optician, but at the same time a mechanical engineer of no ordinary ability. Thus he seems to have been the first person to apply the ordinary means and machinery of mechanical engineering to the construction of optical instruments, treating them as accurate machines rather than as delicate instruments to be constructed solely by the hand. He made his own tools, and for the most part cast his own brass.

During the last five or six years of his life, Mr. Cooke turned his attention to the subject of circular graduation, and, after studying all the best existing machines for executing that most delicate kind of work, constructed for his own use a self-acting dividing engine of nearly 3-feet diameter. In this beautiful machine there are many novelties; the all-important process for ratching the edge of the main circle, the form of the vertical axis on which it revolves, the means of centring the circle to be operated on, and the gear for working the cutting tool, all differing from anything hitherto similarly employed, exhibit innumerable examples of Mr. Cooke's sound mechanical knowledge prompted by his rare fertility of invention. A larger machine, more perfect still, and capable of dividing circles of 4-feet in diameter, was designed and in part constructed by him, but its completion devolves on his successors. The self-acting engine formed part of the plant necessary to enable Mr. Cooke to carry out, under his own eye, the considerable orders for theodolites and other surveying instruments which latterly were given him by the Indian Government. Into this class of instruments he introduced many valuable improvements, and the workmanship which he bestowed on them, as on every thing that came from his hands, has rarely been equalled—never excelled.

Mr. Cooke was a horologist of no mean pretensions. Nearly one hundred turret-clocks, erected in churches and public institutions in all parts of England, testify by the accuracy of their performance to the exactness of their maker's mind. He brought to great perfection astronomical clocks, not only those of the ordinary kind, but also clocks exhibiting, under the regulation of a single pendulum, both sidereal and solar time on the same dial.

There was in Mr. Cooke's temperament no small admixture of the artistic element; a certain grace of form and harmony of proportion will be found to characterise all his works,—ennobling, by beautifying, the structure reared on rigid principles. Nor was he indifferent to the convenience of the too often forgotten observer. He was the first to employ, on the suggestion of his friend Lieut.-Col. Strange, machinery for engraving the

figures on his instruments. By means of a small and simple engine the entire figures, of a standard form of maximum visibility, are engraved on a circle in less than two hours by an ordinary mechanic, with an elegance and precision unattainable by the most skilful unaided hand.

The first important meridional instruments made by Mr. Cooke were a pair of 5-feet transits destined to be employed in the determination of longitude by the officers of the Great Trigonometrical Survey of India. These are probably the largest portable instruments of their class in existence; and in some other respects they are unique. The transit axis are of aluminium bronze, an alloy selected for its immense rigidity; the appliances for effecting the principal adjustments are peculiar; and the mode in which no less than four levels are employed for effecting and watching the horizontality of the axis, and for determining its variations of flexure for different altitudes, contrived exclusively by Mr. Cooke, involves principles and presents features worthy of imitation. These fine instruments are briefly described by Lieut.-Col. A. Strange, under whose superintendence they were constructed, in the *Monthly Notices* for April and May, 1865, and in the *Proceedings of the Royal Society*, No. 90, 1867.

In September, 1863, Mr. Newall, of Gateshead, gave Mr. Cooke the princely commission to construct for him the largest refracting telescope that had ever been proposed with the least probability of a successful issue; it was a gigantic equatoreal of no less than 25 inches clear aperture! At the present time we believe the largest completely mounted equatoreal does not exceed 15 French inches in aperture; the stride from even these magnificent machines to the dimensions of that undertaken by Mr. Cooke is simply enormous. The accurate figuring and polishing of such an object-glass requires an amount of skill, patience, mechanical resource, and self-reliance, of which no person not conversant with the innumerable difficulties besetting the attempt can form a conception. It may be sufficient here to say that in the present instance it was necessary to float the huge lenses of the telescope on quicksilver, lest some undue or unequal pressure during the act of polishing their surfaces should disturb their forms. There were also other mechanical contrivances of great ingenuity, which upon the final completion of the instrument, Mr. Cooke proposed to communicate to the scientific public without reserve. The object-glass was finished early in 1868, and the mounting is now nearly complete. The telescope is driven by a clock, the regulation of which is partly effected by the introduction of a pendulum escapement, producing an uniformity of motion which probably has not heretofore been obtained by other means. The description of this clock will be found in the *Monthly Notices* for 1868. The object-glass is said to be a complete success; if that on further trial prove to be the case, a work will have been accomplished which, for its magnitude and difficulty, is unexampled in the history of Astro-

nomical Instruments. A noble transit-circle of 7 inches aperture is to accompany this imperial philosophical machine.

When Troughton completed his magnificent transit instrument for the Royal Observatory at Greenwich, he inscribed his honoured name upon it with very pardonable pride; in his days it was unique in the world, for all days it will be a noble monument of his skill. But this beautiful work of the great optician would look like a toy by the side of Cooke's gigantic equatoreal. The name of its constructor will ere long be engraved on this mighty instrument; it may well serve as the epitaph for his tomb, for the eye of Thomas Cooke was not permitted to see the completion of the work which his own genius had devised. Worn out, partly by the days and nights of anxiety which were unavoidably incident on so vast an undertaking, and partly by the struggles which always accompany the efforts of a man who has to make his own fortunes unaided in the world, he sank to his rest on the 19th of October, 1868, in the 62nd year of his age.

For some years past the great York factory has been practically superintended by two of Mr. Cooke's sons — Mr. Thomas Cooke, the depository of his father's profound knowledge, an accomplished optician; and Mr. Frederick Cooke, a mechanician of the highest attainments. The traditions and the methods of the house are in safe hands,— in the hands of those who will spare no effort to maintain and augment the fame which has become their most valued inheritance.

Those who had the advantage of knowing Thomas Cooke could not fail to admire him for his attainments and his genius; for the genuine truthfulness and simplicity of his character he won their love. Unlike most self-educated men, he was singularly modest in the appreciation of his own powers; unlike many men who have had long to struggle for eminence in their profession, he was singularly free from the depreciation of the abilities of his rivals. The foundation of his future skill as an optician he had laid in early life by a diligent application to mathematical studies, and it was impossible for any competent person to converse with him on the subject of his favourite science without being struck with his profound knowledge of every branch of its theory, while it was evident that he had put that theory to the test, and in some important instances had improved it by his experiments. It was owing to this happy combination of theory with experiment that the late Mr. Dawes was enabled to pass his well-known and well-deserved eulogy on the peculiar character of the achromatism of his telescopes.

England has acquired a well-merited reputation for its long and unequalled succession of eminent constructors of astronomical instruments. To the great names of the past, such as Dollond, Bird, Ramsden, Troughton and Simms, we must now in gratitude add that of Thomas Cooke. Happily there are those still living among us who possess the promise and the power of emulating his fame. [C. P.]

Proceedings of various Observatories.

Royal Observatory, Greenwich.

Since the date of the last Report the principal special work at the Royal Observatory has been the reduction of the great bulk of the preparatory work for the new Seven-year Catalogue of Stars, which will contain the results of all the stellar observations made with the Transit-Circle, from 1861, January 1, to 1867, December 31. The star-constants, precessions, and secular variations, have been computed afresh for the adopted epoch (1864), and considerable progress is made in the reduction of the yearly values to the same epoch.

The addition of a labour of this kind to the ordinary office-work of the Observatory is always sufficient to absorb a large proportion of the computing-staff of the establishment. The work is, however, performed without any serious interference with the usual routine computations. It is believed that the new Catalogue will be found to be exceedingly valuable to Astronomers, for it will contain not only the positions of all the standard stars, but also those of a considerable number of stars which had never been observed at Greenwich since the time of Dr. Bradley, and which are included in Bessel's *Fundamenta Astronomiæ*. Every star observed by Bradley, whose R.A. and N.P.D. are contained in that work, has now been re-observed at Greenwich since the Observatory has been under the direction of the present Astronomer Royal. We shall have, therefore, ample materials for the determination of the proper motions of the remaining Bradley's stars not previously included in the papers of the Rev. R. Main and Mr. Stone. The Catalogue will also include stars observed for special purposes, as, for example, at the request of Colonel Sir Henry James, Director of the Ordnance Survey, and of Colonel Walker, Superintendent of the Great Trigonometrical Survey of India.

The number of stars contained in the new Catalogue will be about 2760.

During the past year, the Sun, Moon, planets, and special list-stars, have been observed as usual with the Transit-Circle; and the Moon, with accompanying stars and collimator for the instrumental corrections, with the Altazimuth. The observations with the latter instrument have been very numerous during the past summer and autumn, the number of complete observations of the Moon having far exceeded that in the corresponding period of any previous year.

The printing of the volume of the *Greenwich Observations* for 1867 is in an advanced state, and the reductions of the observations made in 1868 are nearly completed.

The transit of *Mercury* over the Sun's disk on the morning of November 5 was very successfully observed. The details of the observations are given in the *Monthly Notices*. Independently of the value of the observed times of egress, the phenomenon gave the observers an opportunity of noticing the formation of the black ligament, and of deciding the question of the reality of the existence of the *two* phenomena of real and apparent internal contacts, as explained by Mr. Stone, in his recent paper on the 'Rediscussion of the Observations of the Transit of *Venus* in 1769.'

For the express purpose of determining the heating powers of the stars, a delicate combination of a small thermo-electric pile and sensitive reflecting galvanometer has been arranged by Mr. Stone. This apparatus (Mr. Stone's private property) which was finished in 1868, December, has only been tried, on account of late unsettled weather, on two nights; and that merely sufficiently to test its applicability to the purpose for which it was arranged. There was no intention to have called public attention to the matter until quantitative results had been obtained. It now appears that Mr. Huggins had, in 1867, applied a similar arrangement to the same purpose. Under these circumstances it has been thought advisable to mention the perfect independence of Mr. Stone's work upon this subject.

Royal Observatory, Edinburgh.

The number of meteorological stations in Scotland, whose observations are computed here, remains at fifty-five, and these are still regularly prepared for and printed by the Registrar-General of Births, Deaths, &c. in Scotland, either every month, or quarter, or both.

The distribution of time to the public by time-ball, time-gun, and Jones's controlled clocks, continues as heretofore; also meridian observations of stars with Transit instruments and Mural Circle—the zero points for Right Ascension being adapted invariably from the Greenwich list of fundamental stars, both polar and equatorial, as most obligingly furnished by the Astronomer Royal, G. B. Airy, at the beginning of every year.

Radcliffe Observatory, Oxford.

No change has taken place in the organisation or subjects of observation at this Observatory during the past year. The Transit Circle has been actively employed, and the Heliometer whenever the weather would permit it, throughout the year 1868.

The state of the reductions and of the printing is kept up to pretty nearly the same state of forwardness as before, as will be shown by the following statement. All the Astronomical Observations of 1867 have been completely reduced and discussed, and

the Catalogue of Stars, containing about 1400, observed in both elements. As usual, considerable pains have been bestowed upon the discussion of the observations of N.P.D., which are on the whole very satisfactory. The transits have been reduced to the end of the year 1868, but the observations of N.P.D. are less advanced than usual, on account of the labours bestowed on the Second Radcliffe Catalogue. This is now essentially complete, and the printing of it will commence very shortly. An elaborate comparison has been made of the positions of the stars in this Catalogue with the positions of stars in the Greenwich Catalogue of 2022 stars for 1860. The varying errors of the equinox at different hours of Right Ascension are very similar to those in the First Radcliffe Catalogue; and, with regard to the N.P.D.s, the differences are small, but there is a curious law of periodical errors (the stars being arranged in order of polar distance) very similar to that exhibited in the observations made with the Carrington Transit Circle. Thus we have observations made with two distinct circles, and treated by two different astronomers, giving almost precisely the same errors when compared with the results of the Greenwich Transit Circle; and the differences are not accounted for in any great degree by the difference of the refraction constants which are employed in the reductions.

The printing has gone on at the usual rate, that is, the results of the Astronomical Observations for 1866 have been printed for some time, and the printing of the volume for 1867 has been begun. The Meteorological Reductions for 1866 are nearly finished, and then the volume will be completed.

While speaking of the meteorology, it may be mentioned that the observations of the wind for some years have been recently discussed by Mr. Baxendell of Manchester, who has arrived at a very curious result with reference to the well-known fact of the different average velocities of the wind during the day and night hours. Observing that during the night hours, till seven o'clock in the morning, the velocity is sensibly constant, but increasing very rapidly after that hour, he finds that the force put in action to produce this additional velocity is almost precisely at right angles to the direction of the magnetic meridian; and he considers that this force, from whatever cause derived, is too large to be neglected in any speculations or forecasts of the weather.

Cambridge Observatory.

The Transit and Circle Observations during the year have been limited almost wholly to the usual observations of the Sun about the times of the equinoxes and solstices; to observations of *Jupiter*, *Uranus*, *Neptune*, and *Iris*, near the oppositions, and to the observations of fundamental stars for the determination of instrumental and clock errors. *Iris* was observed for the purpose of testing the accuracy of the Ephemeris, which Dr. Brunnow has computed by means of his new and still unpub-

lished Tables of that planet. These observations were made with great difficulty on account of the deficiency in optical power of our present meridian instruments.

The errors due to the form of the pivots of the Transit instrument have been carefully re-determined. A comparison with former determinations shows that the changes arising from the wear of the pivots are very slight.

Two very satisfactory series of observations of Brorsen's and Winnecke's Comets were taken with the Northumberland Equatoreal. The relative position and distance of *Algol* and its companion were determined at the request of Mr. Aldis.

The Equatoreal has also been used occasionally in observing occultations.

The courses of a considerable number of the August Meteors were observed by means of the two meteoroscopes.

The anemometer has been out of order more than once during the past year in consequence of the formation of ice in the interior of the box containing the wheel-work, by which the wheels have been stopped and their teeth injured. Measures have now been taken to prevent the accumulation of moisture in the interior for the future. With the exception of these interruptions in the determination of the velocity of the wind, the meteorological observations have been punctually registered.

The reductions and the preparation for the press have gone on steadily. The Transit Observations of several years are ready for the press, and part of the copy is in the printer's hands. Considerable progress has also been made in the copy for the press of the Mural Circle observations.

Nearly all the Equatorial observations of occultations and of comets made up to the present time are reduced.

The Meridian circle which Mr. Simms is constructing for this Observatory is now in a very forward state, and it is hoped that it will prove to be one of the finest instruments of its class. Preparations will shortly be made for mounting this instrument, which is to occupy the place of the present Transit instrument.

Liverpool Observatory, Bidston, Birkenhead.

An important alteration has recently been sanctioned by the Mersey Docks and Harbour Board with regard to the conditions on which chronometers are in future to be received at the Liverpool Observatory. Hitherto they have been left for an indefinite time, for the purpose of being rated, and a large amount of labour has thereby been thrown upon the observatory. In future, chronometers are to be received for such a time only as is found to be necessary for efficiently testing them. Subject to the regulations, chronometers belonging to ships paying dues to the Mersey Dock Estate are tested gratuitously, as are also those belonging to any chronometer-maker, who on application can now send such a num-

ber to be tested as the Director of the Observatory may from time to time decide upon.

As a means of communicating time to the port and towns of Liverpool and Birkenhead the time-gun is found to be very successful. The position of the gun is such that a view of the flash is commanded for several miles both up and down the river Mersey, and the sound is well heard for a long distance on each side of the river.

The work in the hands of Messrs. Troughton and Simms has been greatly delayed, and is not yet completed.

Glasgow Observatory.

The observations at the Glasgow Observatory during the past year have been essentially the same as in former years. The Transit Circle is employed in the observation of a selected list of stars below the fifth magnitude. Greenwich time continues to be transmitted to the city and port of Glasgow by Jones's method of control. An improved form of micrometer by Troughton and Simms has been recently obtained for the Ochtertyre Equatoreal, and observations of the minor planets are now made with the instrument on every favourable night. A few observations of Brorsen's Comet were obtained on the occasion of its last return to the perihelion. Professor Grant finally reports that the system of meteorological operations established by the Meteorological Committee of the Royal Society, acting in concert with the Board of Trade, is now regularly prosecuted at the Observatory.

Royal Observatory, Cape of Good Hope.

A very careful series of observations of *Mercury* on the Sun's disk were made here by Mr. Mann. A very accurate determination of the errors of the differences in the tabular places of the planet and the Sun is thus afforded.

The November Meteors were well observed. These results will shortly appear in our *Notices*.

The reductions for a Catalogue are going on unflinchingly.

Kew Observatory.

During the past year the Kew Heliograph has been at work without interruption, except from unfavourable weather.

There were on the whole 174 days of observation, on which 285 sun-pictures were taken.

The measurements alluded to in the last Report are continued with the utmost rapidity consistent with that extreme accuracy which is necessary in determining the positions of spots, and at the present moment the heliographic positions, as well as the areas of three years of sun-spots, have been determined, commencing

with February, 1862. The measurements for two of these three years are given in a paper (alluded to in the last Report), which was presented to the Royal Society on March 31st, 1868, and is entitled "Researches on Solar Physics; Heliographical Positions and Areas of Sun-spots, observed with the Kew Heliograph during the years 1862 and 1863." By Messrs. De La Rue, Stewart, and Loewy. The paper is at the present moment going through the press as part of the *Transactions of the Royal Society.*

The same authors communicated to the Royal Society in June, 1868, an account of certain observations and measurements of a spot which was fortunately photographed at Kew while in the act of passing round the Sun's limb. This paper is entitled "An Account of some recent Observations on a Sun-spot made at the Kew Observatory," and is also being printed in the *Transactions of the Royal Society.*

Besides these two papers there has been printed a short appendix to the second series of preliminary researches in Solar Physics. This paper exhibits the distribution in heliographic latitudes of the sun-spots observed by Carrington, and shows, on an average, the latitudes of sun-spots when *Venus* crosses the solar system as less than when she is at her greatest distance therefrom. It also shows that there is excessive solar action at those periods when *Venus* and *Mercury* are together, thus tending, in connexion with previous researches, to the conclusion that *Venus*, *Jupiter*, and *Mercury*, are the planets most influential as regards the production of spots.

Finally, two communications have been made to the Royal Astronomical Society; the one giving a representation of the appearance which the Sun presented at Kew on the day of the total eclipse, Aug. 18, 1868, and the other being the usual yearly comparison of the number of groups of spots observed at Kew and at Dessau during the past year.

The plan of work for the present year is, while continuing the photographic observations, to push on with the measurements; and it is hoped that before next Report the positions and areas of spots for two more years may have been made, thus comprising five years in all, from the commencement in 1862. And also that another paper, embodying these positions and areas, may have been communicated to the Royal Society.

It is also hoped that the observations on distortion may be repeated and reduced. At the present moment it is impossible to commence the series of actinic observations alluded to in last Report.

Lord Rosse's Observatory.

During the last year the observations on the nebulæ and clusters of stars in Sir John Herschel's Catalogue have been continued in the same manner as heretofore.

Lord Rosse's present assistant has on two or three occasions examined the spectrum of the Great Nebula in *Orion*, and confirms previous suspicion that there exist in it other bright lines in addition to the three well-known ones; more particularly one on the more refrangible side of F (a position where it could not be confounded with the maximum brightness of a continuous spectrum); all these lines are, however, of extreme faintness.

During this season the planet *Jupiter* has been examined, and a few sketches made.

A series of drawings of *Mars* has also lately been begun.

Some experiments on lunar radiation have been commenced, which have been so far successful as to show that the thermo-electric pile is capable of detecting heat near the time of full moon. In consequence, however, of the various disturbing causes having been only partially eliminated, as yet no reliable estimate of its amount has been made; the hope is entertained that this may be done.

Mr. Huggins' Observatory.

Time has not been found during the past year to continue some experiments which were conducted in the Observatory in the early part of 1867, for the purpose of detecting the heat received from the stars, and also, if possible, from the Moon. It may be well, however, to state that though, in consequence of the great difficulty of avoiding disturbing influences, no trustworthy quantitative estimate was obtained, yet, in the case of the stars *Sirius*, *Regulus*, *Pollux*, and *Arcturus*, the observations made in the early part of 1867 showed that the very sensitive thermopile, which had been specially constructed for these experiments, was sensibly affected. The heat of the Moon was observed with less certainty. These observations will be continued.

In the early part of 1868, observations which had been for some time in progress for the purpose of ascertaining the motion of stars towards or from the Earth by means of alterations in the refrangibility of their light, were successfully concluded for *Sirius*.* The line in the spectrum of *Sirius*, at the position of Fraunhofer's F, and due to hydrogen, was found to have been shifted by the star's motion towards the red, to an amount such as to show that *Sirius* was moving from our system with a velocity of about 29 miles per second. But this amount has to be corrected for the solar motion in space.

Similar observations on the Great Nebula in *Orion* showed that the coincidence of two of the lines of its spectrum respectively, with a line of hydrogen and a line of nitrogen, was perfectly maintained in an apparatus in which a difference of wave length of 0·0460 millionth of a millimetre would have been

* See *Philosophical Transactions*, 1868, p. 529.

detected. This nebula, therefore, was not receding with a velocity so great as 10 miles per second, for this motion added to that of the Earth would have caused a want of coincidence that would have been observed. If the nebula were approaching us, it might have had a velocity as great as 20 or 25 miles per second, as part of it would have been annulled by the Earth's motion in the opposite direction.

The method of placing the induction spark before the object-glass brought into notice a fact of some interest. The loss of brightness showed itself in the case of the spectra of hydrogen and nitrogen, by the total extinction of all the lines, with the exception of the one line in each spectrum, which is found in the nebulæ. If we should have reason to believe that lines in the nebulæ have been quenched on their way to us, we must consider it as an evidence of a power of extinction of light existing in cosmical space, and not as an effect of distance, since the nebulæ are objects of sensible size.

During the year two Comets have been subjected to prismatic examination. Brorsen's Comet gave a spectrum of three bright bands.* Comet II. 1868, gave also three luminous bands, which, though in similar parts of the spectrum, differed considerably in position and in character from those of Brorsen's Comet. The spectrum of shaded bands of Comet II. was found, by direct comparison in the instrument with the spectrum of olefiant gas, to be identical in refrangibility and in general characters with the spectrum of carbon.†

On April 15, 1868, the spectrum of a solar spot was examined. It was found that three-fourths of the apparent light of the umbra came from that region of the Sun, the remaining fourth from our atmosphere. Most of the lines were increased in width, but this was not the case with the lines C and F, which are due to hydrogen. This fact is of interest in connexion with what we now know to be the nature of the solar prominences, and may have been due to these objects lying over the umbra of the spot.

With the help of the information of the positions of the bright lines of the solar prominences, furnished by the eclipse of August 1868, these bright lines were found, at the first instant of looking for them, with a small spectroscope, with which they had been sought for previously in vain.

The appearances observed at the Transit of *Mercury* have been described in the *Monthly Notices*. For the purpose of throwing light upon the origin of the bright spot seen upon *Mercury*, an artificial disk of about the same apparent size as *Mercury* was viewed upon the Sun, but no bright spot could be detected upon it.

* *Proceedings of the Royal Society*. vol. xvi. p. 386.
† *Phil. Trans.* 1868.

Mr. Lockyer's Observatory.

Mr. Lockyer's observatory work has of late been exclusively devoted to the Sun, and his recent discoveries have been alluded to in another part of this Report.

Mr. Lockyer's detailed account of his work, communicated to the Royal Society and the Paris Academy of Sciences, includes among other matters,—

I. The determination of the exact position and number of the bright lines observed in the red flames, the phenomena which characterise them, and the substance of which they are principally composed.

II. The determination of the fact that the red flames are merely local heapings up of an envelope which is continuous round the Sun.

III. The approximate determination of the pressures existent in the prominences and at the bottom of the continuous envelope.

IV. A discussion of the bearing of these discoveries on the received theory of the physical constitution of the Sun.

Mr. Lockyer has suggested the term "Chromosphere" for the region or shell of the luminous prominences, which he has shown to completely surround the limb of the uneclipsed Sun.

At the time that the spectroscope revealed to Mr. Lockyer the fact that the prominence spectrum was never absent, and that, in fact, the prominence matter formed a continuous envelope round the Sun, he was not aware that such an envelope had been suggested by previous observers. He has since found, however, that Professor Grant * had based such a suggestion on an elaborate investigation of the eclipses up to and including that of 1842. That Mr. Swan and Herr Littrow afterwards came to the same conclusion from the eclipse of 1851; and that, later still, M. Le Verrier † and Father Secchi both published, after the eclipse of 1860, the idea that there was such a continuous envelope, which is indicated in the discussion by Mr. De La Rue of his admirable photographs.‡

The experimental proof of the truth of these surmises is, however, due to Mr. Lockyer. In his paper it is stated that, under proper instrumental and atmospheric conditions, the spectrum of the chromosphere is always visible in every part of the Sun's periphery; its height, and the dimensions and shapes of several prominences, observed at different times, are given in the paper. One prominence, 3′ high, was observed on the 20th of October, 1868. The possibility of variations in the thickness of this envelope is suggested, and the phenomena presented by the star in *Corona* are referred to.

* *History of Physical Astronomy*, chap. ix.
† *Phil. Trans.* 1862, pp. 405, 406; also Plate XV.
‡ *Cosmos*, vol. xvii. p. 174.

Mr. Lockyer also points out that the line F invariably expands (that the band of light gets wider and wider) as the Sun is approached, and that the C line and the D line do not widen; and he enlarges upon the importance of this fact, taken in connexion with the researches of Plücker, Hittorf, and Frankland, on the spectrum of hydrogen. It is in connexion with this widening out that Mr. Lockyer, in conjunction with Professor Frankland, has been already able to determine the pressure at the surfaces of the chromosphere.

The F line, in fact, appears like an arrow-head resting on the Sun, and Plücker and Hittorf have ascribed such a widening out of the green hydrogen line as is here indicated as due either to temperature or pressure. This widening out has been found to be a real curve of pressure, extending from the highest prominences down to the surface of the photosphere; and the prominences have been determined to be of the most excessive tenuity, and the base even of the chromosphere to be of very much less than the pressure of the Earth's atmosphere.

Messrs. Frankland and Lockyer suggest in their preliminary notes on the chemical extension, so to speak, of the Observatory work, that the facts and experiments seem to show that Kirchhoff's hypothesis is no longer tenable; that, in all probability, the chromosphere is the only extensive envelope, outside the photosphere, and that the region of selective absorption is either situated in the photosphere itself or extremely close to it. Further, that the photosphere is neither solid nor liquid, but gaseous; the most brilliant portions of it giving out bright lines and forming apparently an ever-varying shell—the tops of smaller convection currents carrying matters out of the more intensely absorptive region indicated by Mr. Lockyer in a paper communicated to the Royal Society in 1866.

Notices of some points of Interest connected with the Progress of Astronomy during the past Year.

Solar Eclipse of 1868, *August* 18.

The results obtained by the different observers are of such interest and importance that the principal observations which would not otherwise appear in our Transactions are given in considerable detail in the observers' own words.

It is with great satisfaction that the Council call the attention of the Fellows of the Society to the complete success of their own expedition;—a success for which the Fellows are much indebted to the skill and energy of the Superintendent, Major Tennant.

The Astronomical Society's Expedition.

It will be in the recollection of our Fellows that at the last Anniversary Meeting it was stated that preparations had been made at the recommendation of the Council of our Society for the observation of the Total Eclipse of the Sun in India. The Astronomer Royal took a warm interest in the proposed observations, and addressed the Secretary of State for India on the subject. It was ultimately arranged that the expense of the expedition should be borne jointly by the Government of India and the Imperial Government. The superintendence of the expedition was intrusted to Major Tennant. It is with great satisfaction that the Council is able to announce that Major Tennant has been most deservedly and eminently successful.

The Report of Major Tennant's observations is now in the hands of the Society, and it is intended that it shall appear in the forthcoming volume of the *Transactions*, fully illustrated with facsimiles of the photographs taken at Guntoor, which it is proposed to enlarge photographically, in order that the details of the prominences may be seen more clearly than is possible in the small copies which accompany the paper. Mr. De La Rue, who evinced considerable interest in the expedition and afforded facilities to Major Tennant for familiarising himself with astronomical photography before he started, has undertaken to see that the photographs are properly enlarged and copied.

It is here proper to state that to Major Tennant is due the credit of having first called attention to the peculiarly favourable conditions which would be presented by the Solar Eclipse of August 1868.*

It is only justice also to mention that, as far as regards the part which England took in the observations, it was mainly attributable to the energetic, active, and untiring zeal of Major Tennant, who happened to be in England on leave during the greater part of 1867, and who devoted much time in promoting the observations which, in spite of many difficulties, have been so successfully undertaken and carried out.

It will be recollected that Major Tennant, after consulting with the Astronomer Royal and other Fellows of the Society, undertook the following work. It was most comprehensive, and entailed possibly almost too much responsibility for the director of a single expedition.

1st. The determination of the geographical position of the station. This was successfully accomplished by means of a repeating-circle, although, in consequence of bad weather, there were not many available days between the arrival of the observers and instruments at Guntoor and the day of the eclipse. The position was found to be, Latitude N. $16^{\circ}\ 17'\ 29''{\cdot}23$, and Longitude E. $5^{h}\ 21^{m}\ 48^{s}{\cdot}6$.

* *Monthly Notices*, vol. xxvii. pp. 79, 174.

Captain Branfill, R.E., subsequently connected the station with the marks of the Great Trigonometrical Survey, and deduced the following result: Lat. N. 16° 17′ 34″·3, and Long. E. $5^h\ 21^m\ 46^s{\cdot}5$.

2nd. Spectroscopic Observations. These were undertaken by Major Tennant himself, by means of the Sheepshanks' equatoreal, of 4·6 inch aperture and 5 feet focal length. This had been mounted equatoreally by the late Mr. Cooke, and was suitable for all latitudes in the British Isles, but it had to be altered to suit the more southern stations of India. The spectroscope employed with the telescope was made by Messrs. Troughton and Simms, and was provided with a scale of equal parts, which was illuminated by means of a lamp. The addition of this spectroscope threw additional work on the driving clock beyond that for which it was originally calculated, and, in consequence, some difficulties were experienced just at the critical time of observation from the irregularity of its going.

In spite, however, of this and other mishaps Major Tennant was able to carry out his observations, and ascertained, 1st, that the corona only gave the continuous solar spectrum. 2nd, that the light of the prominences was resolvable into certain bright lines of definite refrangibility, showing that these appendages consist of gaseous matter at a very high temperature. Major Tennant states that the Great Horn gave a beautiful line in the red, a line in the orange, and one in the green, which appeared multiple, also a line seen with difficulty near F; he says the red and yellow lines were evidently C and D, the reading of the bright line coincides with that of the brightest line in *b*. The line near to F was, in all probability, F itself; E, he says, was certainly not seen by him, and that, as regards the line in the blue, it was useless from his data to speculate upon it.

We now have more precise information from the researches of M. Janssen and Mr. Lockyer respecting the position of the bright lines, and the probable nature of the Sun's appendages; but it must be admitted that Major Tennant did this part of his work well, especially when the scope of the instruments at disposal is taken into account.

3rd. Major Tennant noted the time of first contact of the Sun and Moon's limbs by means of the repeating-circle at $6^h\ 2^m\ 12^s{\cdot}60$, sidereal time, and that of the last contact at $8^h\ 47^m\ 24^s{\cdot}62$, sidereal time.

4th. Polariscope. This part of the work was most ably performed by Captain Branfill, who joined Major Tennant early in August, and immediately set to work to familiarise himself with the phenomena produced by polarised light in the telescope. This instrument was one of the old collimators of the Great Transit Circle of Greenwich, and was lent by the Astronomer Royal. It was mounted on a polar axis, so that with one movement it could be made to follow the apparent motion of the Sun, but it was not provided with a driving-clock. To this telescope

a polariscope eye-piece had been fitted by Mr. Ladd. The polarising apparatus comprised several combinations which could readily and rapidly be substituted one for the other. All these concurred in showing that the prominences (the Great Horn was chiefly observed) gave no indication of polarised light; on the other hand, every arrangement brought out the fact that the light of the corona was polarised in a plane passing through the Sun's centre. These observations were therefore fully and successfully carried out, and left nothing to desire.

5th. We now come to the photographic observations; these were under the immediate direction of Sergeant Phillips, who is not only a skilled photographer, but also had the advantage (as well as the Sappers who aided him) of working in Mr. Warren De La Rue's observatory at Cranford. The telescope employed is a Newtonian with a silvered-glass mirror, 9 inches in diameter. by With, and specially mounted by Mr. Browning. Preparations had been made for having a very large field, in order that the corona might be depicted as well as the prominences. Unfortunately the Sun was covered with cumulo-stratus clouds which diminished the actinic power of the light of the corona so much that it was not recorded. In other respects the photographs (six in number) were eminently successful.

Interval. s			h m s		
58·5	No. 1	Exposed at	7 17 2·9	S. T. for	1ˢ
61·4	„ 2	„	18 1·4	„	5ˢ
58·1	„ 3	„	19 2·8	„	10ˢ
63·2	„ 4	„	20 0·9	„	5ˢ
90·7	„ 5	„	21 4·1	„	1ˢ
	„ 6	„	7 22 34·8	„	1ˢ

Paper copies of these, about 2 inches in diameter, accompany the Report; and Mr. James, one of Major Tennant's assistants, made excellent drawings of the Great Horn and other prominences seen in the photograph, by means of the microscope. Since the arrival of the memoir, Serjeant Phillips has brought safely to England eight sets of transparent copies on glass, which have been distributed to individuals and learned bodies; amongst others, to the Royal Society and the French Academy of Sciences. On the occasion of a lecture given by Prof. Herschel, at the Royal Institution, on January 22, these were shown by means of the electric lamp, and projected on a screen, on a scale of about 5 feet for the Moon's diameter. The amount of detail visible under these circumstances was very remarkable. The spiral structure of the Great Horn, to which Major Tennant has called attention, was very evident. This spiral formation Major Tennant ascribes to the confliction of an ascending current, and at right angles to it. Since then Mr. Warren De La Rue

procured some very beautiful copies, about 6½ inches diameter. He has also discussed, graphically, the small paper photographs, and communicated the results to the Society.* The diagram accompanying his paper shows fairly the form and relative position of these appendages with respect to the Sun. Mr. Warren De La Rue thought that he had detected a rotation of the Great Horn on its axis during the intervals between occurrence of totality at the various stations along the line of the eclipse. He has since been favoured by Prof. Foerster, Director of the Berlin Observatory, with a copy of the first Aden photograph, and informs the Council that there does not appear to be any very great change in the appearance of the Great Horn at Aden and at Guntoor. This comparison of results brings out forcibly the great value of photography for this class of observations, for the most careful and collected observer is liable to make an error in recording eye-observations.

In justice, however, to Col. Addison, whom Major Tennant had induced to make observations at Aden, it must be stated that a drawing of the prominences which he sent to Major Tennant led that gentleman to conclude that no change had occurred in them between the epoch of Aden and that of Guntoor.

Major Tennant reached Aden on the 25th of January; as this was nearly the first place where observations of the totality could be made, he enlisted the services of Captain Davis, the Peninsular and Oriental Company's agent, and of an old companion, Major Napier, R.A. Both these gentlemen promised their aid, and he learnt from them that Col. Addison and Major Weir, H. M. 2nd Royal Regiment, would be likely to be valuable coadjutors. Unfortunately, the contemplated observations with the polariscope, spectroscope, and intended drawings of the corona, were rendered impossible, in consequence of clouds. But the prominences were seen and recorded with great accuracy, as it has been before stated.

The Council have every reason to feel satisfied with the steps they took in conjunction with the Astronomer Royal, in furthering Major Tennant's views, and in thus securing a most valuable series of observations.

Lieut. J. Herschel's Account. Position, Jamkandi.

The totality commenced unseen. "A few seconds more and the spectrum of diffuse light vanished also, and told me the eclipse was total, but behind a cloud. I went to the finder, removed the dark glass, and waited, how long I cannot say, perhaps half a minute. Soon the cloud hurried over, following the Moon's direction, and therefore revealing, first, the upper limb, with its scintillating corona, and then the lower. Instantly I marked a

* *Monthly Notices*, vol. xxix. p. 73.

prominence near the needle-point, an object so conspicuous that I felt there was no need to take any precautions to secure identification. It was a long finger-like projection from the (real) lower left-hand portion of the circumference. A rapid turn of the declination-screw covered it with the needle-point, and in another instant I was at the spectroscope. A single glance, and the problem was solved.

"*Its Spectrum.*—Three vivid lines, red, orange, blue; no others, and no trace of a continuous spectrum.

"When I say the problem was solved, I am, of course, using language suited only to the excitement of the moment! It was still very far from solved, and I lost no time in applying myself to measurement. And here I hesitate, for the measurement was not effected with anything like the ease and certainty which ought to have been exhibited. Much may be attributed to haste and unsteadiness of hand, still more to the natural difficulty of measuring intermittent glimpses; but I am bound to confess that these causes were supplemented by a failure less excusable. I have no idea how those five minutes passed so quickly! Clouds were evidently passing continually, for the lines were only visible at intervals—not for one half the time certainly—and not always bright, but still I ought to have measured them all. My failure was in insufficient illuminating power, but why, I cannot tell. I never experienced any difficulty of the kind with the nebulæ, which required that I should flash in light suddenly over and over again. I had found the hand-lamp the surest way, but it failed me here in great measure. The red line must have been less vivid than the orange, for after a short attempt to measure it, I passed on to secure the latter. In this I succeeded to my satisfaction, and accordingly tried for the blue line. Here I was not so successful. The glimpses of light were rarer and feebler, the line itself growing shorter, and what remained of it further from the cross. I did, however, place the cross wires in a position certainly very near the true one, and got a reading before the re-illumination of the field told me that the Sun had reappeared on the other limb. These readings were called out, as those of the solar lines had been, to my recorder, and it was only afterwards that I compared them.

"I need not dwell on the feelings of distress and disappointment which I experienced on realizing the fact that the long-anticipated opportunity was gone, and, as it seemed to me then, *wasted*. I seemed to have failed entirely. Almost mechanically I directed the telescope to the bright limb, to verify the read ings of the solar lines, and in doing so my interest was aga awakened by the near coincidence, as it seemed, of the line with the position of the wires; but a little reflection convinc me that the distance of the former was greater than the e which I might have made in intersecting the blue line. I F, and then D and C. The following were my readings up down:—

	C	D	*b*	F
Before	1·91	2·96	4·58	5·64
	1·90	2 94	4·56	5·61
	1·93	2·98	4·60	5·65
	1·92	2·97	4·58	5·62
Bright lines		[3·00]		[5·56]
After	1·93	3·00		5·65

"I consider that there can be no question that the orange line was identical with D, so far as the capacity of the instrument to establish any such identity is concerned. I also consider that the identity of the blue line with F is not established; on the contrary, I believe that the former is less refracted than F, but not much. With regard to the red line, I hesitate very much in assigning an approximate place; B and C represent the limits, it might have been near C; I doubt it being so far as B. I am not prepared to hazard any more definite opinion about it. Its colour was a bright red. This estimate of its place is absolutely free from any reference to the origin of the lines C and F."

The spectrum of the corona does not appear to have been specially examined.

Lieut. Campbell's Report.

"The instruments in question were as follows: a telescope of 3-inch aperture, mounted on a rough double axis, admitting of motions in azimuth and altitude by hand only unaided by any appliance for clamping and slow motion. The telescope was provided with three eye-pieces of magnifying powers 27, 41, and 98; and with it were furnished two analysers for polarized light, viz., a double-image prism and a Savart's polariscope.

"On the first opportunity after the commencement of the total phase of the eclipse I turned on the double-image prism with the eye-piece of 27 magnifying power, as recommended in the Instructions, which gave a field of about 45′ diameter. A most decided difference of colour was at once apparent between the two images of the corona; but I could not make certain of any such difference in the case of a remarkable horn-like protuberance, of a bright red colour, situated about 210 degrees from the vertex, reckoned (as I have done in all cases) with reference to the actual, not the inverted image, and with direct motion. I then removed the double-image prism and applied Savart's polariscope, which gave bands at right angles to a tangent to the limb, distinct, but not bright, and with little, if any, appearance of colour. On turning the polariscope in its cell, the bands, instead of appearing to revolve on their own centre, passing through various phases of brightness, arrangement, &c. travelled bodily along the limb, always at right angles thereto, and without much change in intensity, or any at all in arrange-

ment. The point at which they seemed strongest was about 140° from the vertex, and I recorded them as black centred. Believing that with a higher power and a smaller field I should find it easier to fix my attention on one point of the corona, and observe the phases of the bands at that point, I changed eye-pieces, applying that of 41 power. With this eye-piece the first clear instant showed the bands much brighter than before, coloured, and as tangents to the limb at a point about 200° from the vertex: but before I could determine anything further a cloud shut out the view, and a few seconds later a sudden rush of light told that the totality was over, though it was difficult to believe that five minutes had flown by since its commencement. I experienced a strong feeling of disappointment and want of success: the only points on which I can speak with any confidence being as follows:—

"(1.) When using the double-image prism, the strong difference of colour of the two images of the corona, and the absence of such difference in the case of the most prominent red flame. (2.) With Savart's polariscope the bands from the corona were decided: with a low power they were wanting in intensity and colour: excepting alternate black and white, making it difficult to specify the nature of the centre: and their position was at right angles to the limb, extending over about 30° of the circumference. When the polariscope was turned the bands travelled bodily round the limb without other changes in position or arrangement, as if, indeed, they were revolving round the centre of the Sun as an axis. With a higher power, when a smaller portion of the corona was embraced, the bands were brighter coloured, and seen in a different position, viz., tangents to the limb.

"The appearance observed with a low power seems exactly what might be expected supposing the bands to be brightest at every point when at right angles to the limb, in which case the bands growing into brightness at each succeeding point of the limb, would distract attention from those fading away at the points passed over as the analyzer revolved."

Rapport de M. Rayet.

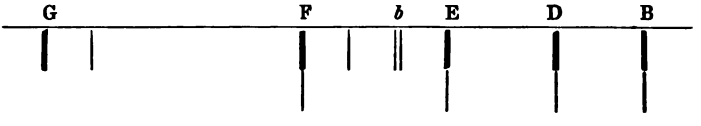
"L'appareil dont je me suis servi à Wah-Tonne, pour l'examen optique de la lumière des protubérances, se composait d'un télescope à miroir de verre argenté, de 20 centimètres de diamètre, monté équatorialement pour la latitude de la station, et d'un spectroscope à vision directe. Ce dernier instrument, formé de trois prismes très-dispersifs, était combiné pour avoir une faible longueur et donner beaucoup de lumière.

"Spectre des cornes.—La fente du spectroscope ayant été orientée de manière à couper à angle droit l'image du croissant lumineux très-étroit qui devait rester quelques secondes avant la sécurité totale, j'ai d'abord étudié la lumière de l'extrémité

cornes. Sur le fond d'un spectre à raies obscures très-nettes, formé par la lumière atmosphérique diffuse, on voyait une bande beaucoup plus lumineuse, qui était le spectre de la lumière émise par l'extrémité de la corne. Quelque fût le peu de hauteur de cette partie on n'y distinguait rien de particulier. Les raies avaient un aspect (largeur et degré d'intensité) identique à celui des raies du spectre solaire ordinaire.

" L'observation des cornes a toutefois été interrompue quelques secondes avant l'éclipse totale, afin d'enlever les diaphragmes mis au téléscope, d'ouvrir légèrement la fente du spectroscope et d'être ainsi préparé à l'examen des protubérances.

" Spectre des Protubérances.—Dès l'instant de l'obscurité totale, la fente du spectroscope ayant été portée sur l'image de la longue protubérance qui venait de se montrer sur le bord oriental du Soleil, je vis immédiatement une série de neuf lignes brillantes, qui, d'après leur disposition dans le champ, leur espacement relatif, leur couleur et enfin par la physionomie même de leur ensemble, me semblent devoir être assimilées aux lignes principales du spectre solaire, B, D, E, *b*, une ligne inconnue, F, et deux lignes du groupe G. Ces lignes présentaient un très-grand éclat, et se détachaient vivement sur un fond gris cendré très-pâle.



" Les protubérances sont donc des jets d'une matière gazeuse incandescente, les flammes d'un phénomène chimique d'une puissance extrême. Il faut aussi remarquer que la lumière de la couronne est très-faible par rapport à celle des protubérances; car, tandis que la lumière de ces dernières donnait un spectre très-vif, la première, malgré l'ouverture assez grande de la fente, ne donnait aucun spectre coloré sensible.

" Pendant les observations précédentes, la fente du spectroscope était parallèle à la grande longueur de la protubérance; aussi voyait-on dans l'appareil des lignes lumineuses d'une hauteur sensible, en relation directe avec la hauteur de la protubérance. La fente ayant été tournée de 90 degrés, les raies se sont trouvées réduites à l'apparence de points brillants répondant à la faible largeur de la corne lumineuse. Il n'y a donc pas d'erreur d'observation possible, les lignes brillantes représentent bien le spectre de la lumière des protubérances.

" Le spectroscope étant dans la première position (fente parallèle à la longueur de la protubérance), les lignes très-vives, assimilées à D, E, et F, se prolongeaient au delà de la longueur moyenne par un trait lumineux très-faible; le spectre présentait l'apparence reproduite dans le dessin ci-joint. Une certaine portion de la matière gazeuse incandescente qui forme les pro-

tubérances se répand donc dans l'atmosphère solaire au delà des limites que l'œil assigne en général à ces expansions.

" L'examen de cette première protubérance étant terminé, j'ai mis la fente sur la grande région lumineuse qui était à l'occident du Soleil. Le spectre s'est, cette fois encore, montré formé des lignes brillantes, disposées comme dans le premier cas, seulement je n'ai vu qu'une seule ligne violette. Toutes les protubérances ne semblent donc point émettre une lumière identique.

" M. Hall, ingénieur-hydrographe de la Marine de Saïgon, qui observait en un autre point de notre station, a également constaté que le spectre des protubérances était formé des lignes brillantes."

Rapport de M. Janssen.

" *Première partie.*—Le paquebot des Messageries impériales qui m'amenait de France m'a débarqué à Madras, le 16 Juillet, sur la côte de Coromandel.

"A Madras, j'ai été reçu par les autorités anglaises avec une grande courtoisie. Lord Napier, gouverneur de la province de Madras, me fit conduire à Masulipatam, sur un vapeur de l'Etat. M. Graham, collecteur adjoint, fut attaché à ma mission pour aplanir toutes les difficultés que je pourrais rencontrer dans l'intérieur.

" Il me restait à choisir ma station.

" Si on jette les yeux sur une carte de l'éclipse, on voit que la ligne de la centralité, après avoir traversé le golfe du Bengale, pénètre sur la côte Est du continent Indien à la hauteur de Masulipatam ; elle coupe les bouches de la Kistna, traverse de grandes plaines formées par le delta de ce fleuve et s'engage ensuite dans un pays élevé, contenant plusieurs chaînes situées à la frontière de l'Etat indépendant du Nizam.

"D'après l'ensemble des informations très-nombreuses recueillies et discutées, je fus conduit à choisir la ville de Guntoor, placée sur la ligne centrale à égale distance des montagnes et de la mer; j'évitais ainsi les brumes marines très-fréquentes à Masulipatam, et les nuages qui couronnent souvent les pics élevés.

"Guntoor est une ville indienne assez importante, centre d'un grand commerce de cotons. Ces cotons viennent en majeure partie des Etats du Nizam, et passent en Europe par les ports de Cocanada et Masulipatam. Plusieurs familles de négociants français résident à Guntoor ; elles descendent, pour la plupart, de ces anciennes et nombreuses familles qui, au siècle passé, faisaient fleurir nos belles colonies de l'Inde.

" Mon observatoire fut établi chez M. Jules Lefaucheur, qui voulut bien mettre à ma disposition tout le premier étage de sa maison, la plus élevée et la mieux située de Guntoor. Les pièces de ce premier étage communiquaient avec une large terrasse sur laquelle je fis élever une construction provisoire répondant aux exigences de nos observations.

" Mes instruments consistaient en plusieurs grandes lunettes de 6 pouces d'ouverture et un télescope Foucault de 21 centimètres de diamètre.*

" Les lunettes étaient montées sur un même plateau qui les rendait solidaires. Le mouvement général était communiqué par un mécanisme construit par MM. Brunner frères, qui permettait de suivre le Soleil par un simple mouvement de rappel. L'appareil était muni de chercheurs de 2 pouces et $2\frac{3}{4}$ pouces d'ouverture formant eux-mêmes de bonnes lunettes astronomiques.

" En analyse spectrale céleste, les chercheurs ont une importance toute particulière; c'est par leur intermédiaire qu'on sait sur quel point précis de l'objet étudié se trouve la fente du spectroscope de la lunette principale. Il importe donc que les fils reticulaires, ou en général les points de repère placés dans le champ du chercheur, soient réglés très-rigoureusement sur la fente de l'appareil spectral. Tous mes soins avaient été apportés pour atteindre ce but capital. Des micromètres spéciaux devaient permettre, en outre, de mesurer rapidement la hauteur et l'angle de position des protubérances. Quant aux spectroscopes adaptés aux grandes lunettes, je les avais choisis de pouvoirs optiques différents, afin de pouvoir répondre aux diverses exigences des phénomènes de l'éclipse. Enfin, tout l'appareil † portait, du côté des oculaires, des écrans en toile noire formant chambre obscure, et destinés à conserver à la vue toute sa sensibilité.

" Indépendamment de ces instruments, consacrés à l'observation principale, j'avais apporté une riche collection de thermomètres d'une grande sensibilité, construits avec talent par M. Baudin,‡ des lunettes portatives, des hygromètres, baromètres, etc. Aussi ai-je pu utiliser le bon vouloir de MM. Jules, Arthur et Guillaume Lefaucheur, que se mirent à ma disposition pour les observations secondaires. M. Jules Lefaucheur, exercé au maniement du crayon, se chargea du dessin de l'éclipse. Une excellente lunette de 3 pouces, munie de réticules, fut mise à sa disposition; il s'en servit d'avance et s'exerça, sur des représentations artificielles d'éclipses, à reproduire d'une manière rapide et sûre les phénomènes qu'il aurait à représenter. La mesure des températures fut confiée à M. Arthur Lefaucheur, qui devait aussi au moment de la totalité, par une expérience très-simple de photométrie, nous faire connaître le pouvoir lumineux des protubérances et de l'auréole.

" J'étais assisté dans mes observations propres par M. Rédier, jeune aspirant au grade d'officier, que M. le commandant du

* " Le miroir de ce télescope avait été parabolisé par M. Martin, qui a voulu donner un concours désintéresse à notre expédition."

† " MM. Bardou et Secretan m'avaient obligeamment prêté deux des quatre objectifs de 6 pouces que j'avais avec moi. M. Bardou m'avait fourni la majeure partie des instruments de cet appareil. Je citerai aussi M. Wentzel pour le talent qu'il montre chaque jour dans le travail de mes prismes."

‡ " Parmi ces thermomètres s'en trouvait un construit sur mes indications par M. Baudin, sur le plan des thermomètres différentiels de M. Walferdin, mais dont le réservoir n'avait pas plus de 1 millimètre de diamètre."

paquebot *l'Impératrice* avait bien voulu mettre à ma disposition. Le concours de M. Rédier, doué d'ailleurs dispositions heureuses pour les sciences d'observation, m'a été fort utile.

" Le temps qui nous resta avant l'éclipse fut employé à des études et des répétitions préliminaires ; elles eurent l'avantage de familiariser tout le monde avec le maniement des instruments et me fournirent l'occasion de nombreux perfectionnements de détail.

" L'éclipse approchait et le temps ne semblait pas devoir nous favoriser. Il pleuvait depuis longtemps sur toute la côte. On considérait ces pluies comme exceptionnelles. Bien heureusement le temps se remit peu à peu avant le 18. Le jour de l'éclipse, le Soleil brilla dès son lever, bien qu'il fût encore dans une couche de vapeurs ; il s'en dégagea bientôt, et au moment où nos lunettes nous signalaient le commencement de l'éclipse, il brillait de tout son éclat.

" Chacun était à son poste. Les observations commencèrent immédiatement.

" Pendant les premières phases, quelques légères vapeurs vinrent passer sur le Soleil ; elles nuisirent à la netteté des mesures thermométriques ; mais quand le moment de la totalité approcha, le ciel reprit une pureté suffisante.

" Cependant la lumière baissait visiblement; les objets semblaient éclairés par un clair de Lune. L'instant décisif approchait, et on l'attendait avec une certaine anxiété : cette anxiété n'ôtait rien à nos facultés, elle les surexcitait plutôt ; et d'ailleurs elle se trouvait bien justifiée, et par la grandeur du phénomène que la nature nous préparait, et par le sentiment que les fruits de longs préparatifs et d'un grand voyage allaient dépendre d'une observation de quelques instants.

" Bientôt le disque solaire se trouve réduit à une mince faucille lumineuse. On redouble d'attention. Les fentes spectrales de l'appareil des 6 pouces sont rigoureusement tenues en contact avec la portion du limbe lunaire, que va éteindre les derniers rayons solaires de manière que ces fentes soient amenées par la Lune elle-même dans les plus basses régions de l'atmosphère solaire quand les deux disques seront tangents.

" L'obscurité a lieu tout à coup, et les phénomènes spectraux changent aussitôt d'une manière bien remarquable.

" Deux spectres formés de cinq ou six lignes très-brillants, rouge, jaune, verte, bleue, violette, occupent le champ spectral et remplacent l'image prismatique solaire qui vient de disparaître. Ces spectres, hauts d'environ 1 minute, se correspondent raie pour raie; ils sont séparés par un espace obscur où je ne distingue aucune raie brillante sensible.

" Le chercheur montre que ces deux spectres sont dus à deux magnifiques protubérances qui brillent maintenant à droite et à gauche de la ligne des contacts où vient d'avoir lieu l'extinction. L'une d'elles surtout, celle de gauche, est d'un hauteur de plus de 3 minutes ; elle rappelle la flamme d'un feu de forge sortant

avec force des ouvertures du combustible, poussée par la violence du vent. La protubérance de droite (bord occidental) présente l'apparence d'un massif de montagnes neigeuses dont la base reposerait sur le limbe de la Lune et qui seraient éclairées par un Soleil couchant. Ces apparences ont été décrites avec soin par M. Jules Lefaucheur; je ferai seulement remarquer, avant de quitter le sujet des protubérances sur lequel j'aurai à revenir d'une manière spéciale, que l'observation précédente montre immédiatement:

"1°. La nature gazeuse des protubérances (raies spectrales brillantes);

"2°. La similitude générale de leur composition chimique (spectres se correspondant raie pour raie);

"3°. Leur espèce chimique (les raies rouge et bleue de leur spectre n'étaient autres que les raies C et F du spectre solaire caractérisant, comme on sait, le gaz hydrogène).

"Je reviens maintenant à l'espace obscur qui séparait les deux spectres protubérantiels. On se rappelle qu'au moment de l'obscurité totale les fentes spectrales étaient tangentes aux deux disques solaire et lunaire; elles traversaient donc les régions circumsolaires immédiatement en contact avec la photosphère, régions où la théorie de M. Kirchhoff place l'atmosphère de vapeurs qui produisent par absorption élective les raies obscures du spectre solaire. Cette atmosphère de vapeurs, quand elle brille de sa lumière propre, doit, suivant la même théorie, donner le spectre solaire renversé, c'est-à-dire uniquement formé de raies brillantes. C'est le phénomène que nous attendions, ou du moins que nous cherchions à vérifier, et c'est pour rendre cette vérification décisive que j'avais accumulé tant de précautions. Mais on vient de voir que les protubérances seules donnèrent des spectres positifs ou à raies brillantes. Or, il est bien constaté que si une atmosphère formée de vapeurs de tous les corps qu'on a reconnus dans le Soleil existait réellement autour de la photosphère, elle eût donné un spectre au moins aussi brillant que celui des protubérances, formées de gaz beaucoup plus subtils et dès lors moins lumineux. Il faut donc admettre, ou que cette atmosphère n'existe pas, ou que sa hauteur est si faible qu'elle a échappé aux observations.

"Je dois dire, au reste, que ce résultat m'a peu surpris. Mes études sur le spectre solaire m'avaient amené à douter de la réalité d'une importante atmosphère autour du Soleil, et je suis de plus en plus porté à admettre que les phénomènes d'absorption élective rejetés par le grand physicien d'Heidelberg dans une atmosphère extérieure au Soleil ont lieu au sein même de la photosphère, dans les vapeurs où nagent les particules solides et liquides des nuages photosphériques. Cette manière de voir serait non-seulement en harmonie avec la belle théorie que nous devons à M. Faye sur la constitution de la photosphère, mais il semble même qu'elle en découle d'une manière nécessaire.

"En résumé, l'éclipse du 18 août a montré, suivant moi, que la constitution du spectre solaire est insuffisamment expliquée par

la théorie admise jusqu'ici, et c'est dans le sens indiqué ci-dessus que je propose de le réviser.

" *Deuxième partie.* —Je reviens maintenant aux protubérances. Pendant l'obscurité totale, je fus frappé du vif éclat des raies protubérantielles : la pensée me vint aussitôt qu'il serait possible de les voir en dehors des éclipses; malheureusement le temps qui se couvrit après le dernier contact ne me permet de rien tenter pour ce jour-là. Pendant la nuit, la méthode et ses moyens d'exécution se formulèrent nettement dans mon esprit. Le lendemain 19, levé à 3 heures du matin, je fis tout disposer pour les nouvelles observations.

" Le Soleil se leva très-beau; aussitôt qu'il fut dégagé des plus basses vapeurs de l'horizon, je commençai à l'explorer. Voici comment je procédai. Par le moyen du chercheur de ma grande lunette, je plaçai la fente du spectroscope sur le bord du disque solaire dans les régions mêmes où la veille j'avais observé les protubérances lumineuses. Cette fente placée en partie sur le disque solaire et en partie en dehors donnait, par conséquent, deux spectres, celui du Soleil et celui de la région protubérantielle. L'éclat du spectre solaire était une grande difficulté ; je la tournai en masquant dans le spectre solaire le jaune, le vert et le bleu, les portions les plus brillantes. Toute mon attention était dirigée sur la ligne C, obscure pour le Soleil, brillante pour la protubérance, et qui, repondant à une partie moins lumineuse du spectre, devait être beaucoup plus facilement perceptible.

" J'étais depuis peu de temps à étudier la région protubérantielle du bord occidental, quand j'aperçus tout à coup une petite raie rouge, brillante, de 1 à 2 minutes de hauteur, formant le prolongement rigoureux de la raie obscure C du spectre solaire. En faisant mouvoir la fente du spectroscope de manière à balayer méthodiquement la région que j'explorais, cette ligne persistait, mais elle se modifiait dans sa longueur et dans l'éclat de ses diverses parties, accusant ainsi une grande variabilité dans la hauteur et dans le pouvoir lumineux des diverses régions de la protubérance.

" Cette exploration fut reprise à trois reprises différentes et toujours la ligne brillante apparut dans les mêmes circonstances. M. Rédier, qui m'assistait avec beaucoup de zèle dans cette recherche, la vit comme moi, et bientôt nous pûmes même en prédire l'apparition par la seule connaissance des régions explorées. Peu après je constatai que la raie brillante F se montrait en même temps que C.

" Dans l'après-midi, je revins encore à la région étudiée le matin; les lignes brillantes s'y montrèrent de nouveau, mais elles accusaient de grands changements dans la distribution de la matière protubérancielle ; les lignes se fractionnaient quelquefois en tronçons isolés qui ne se réunissaient pas à la ligne principale, malgré des déplacements de la fente d'exploration. Ce fait indiquait l'existence de nuages isolés qui s'étaient formés depuis le matin. Dans la région de la grande protubérance je trouvai

quelques lignes brillantes, mais leur longueur et leur distribution accusaient, là aussi, de grands changements.

"Ainsi se trouvait démontrée la possibilité d'observer les raies des protubérances en dehors des éclipses et d'y trouver une méthode pour l'étude de ces corps.

"Ces premières observations montraient déjà que les coïncidences des raies C et F étaient bien réelles, et dès lors que l'hydrogène formait en effet la base de ces matières circumsolaires. Elles établissaient en outre la rapidité des changements que ces corps éprouvent, changements qui ne pouvaient être que pressentis pendant les si courtes observations des éclipses.

"Les jours suivants, je mis à profit toutes les occasions que pouvait m'offrir l'état du ciel pour appliquer la nouvelle méthode et la perfectionner, autant du moins que le permettaient les instruments, qui n'avaient pas été construits à ce point de vue tout nouveau.

"En suivant avec beaucoup d'attention les lignes protubérantielles, j'ai quelquefois observé qu'elles pénètrent dans les lignes obscures du spectre solaire, accusant ainsi un prolongement de la protubérance sur le globe solaire lui-même. Ce résultat était facile à prévoir, mais l'interposition de la Lune en eût toujours rendu la constatation impossible pendant les éclipses.

"Je rapporterai encore ici une observation faite le 4 septembre par un temps favorable, et qui montra avec quelle rapidité les protubérances se déforment et se déplacent.

"A 9^h 50^m l'exploration du Soleil indiquait un amas de matière protubérantielle dans la partie inférieure du disque. Pour en déterminer la figure, je me servis d'une méthode qu'on pourrait appeler chronométrique, parce que le temps y intervient comme élément de mesure.

"Dans cette méthode on place la lunette dans une position fixe, choisie de manière que, par l'effet du mouvement diurne, toutes les parties de la région à explorer viennent successivement se placer devant la fente du spectroscope. On note alors pour chaque instant déterminé la longueur et la situation des lignes protubérantielles qui se produisent successivement. Le temps que le disque solaire met à traverser la fente donne la valeur de la seconde en minute d'arc. Cette donnée, combinée avec la longueur des lignes protubérantielles estimées suivant la même unité,* fournit les éléments d'une représentation graphique de la protubérance.

"L'application de cette méthode à l'étude de la région solaire dont je viens de parler indiquait une protubérance s'étendant sur une longueur d'environ 30 degrés, dont 10 degrés à l'orient du diamètre vertical, et 20 degrés à l'occident. Vers l'extrémité de la portion occidentale, un nuage considérable s'élevait à $1\frac{1}{2}$ minute du globe solaire. Ce nuage long de plus de 2 minutes, large de 1 minute, s'étendait parallèlement au limbe. Une heure après (10^h

* "Cette estimation s'obtient d'une manière facile en plaçant sur la fente du spectroscope deux fils dont l'écartement, réglé sur le foyer de la lunette collectrice, représente un nombre déterminé de minutes d'arc."

50^m), un nouveau tracé montra que le nuage s'était élevé rapidement, prenant la forme globulaire. Mais les mouvements devinrent bientôt plus rapides encore, car dix minutes après, c'est-à-dire a 11 heures, le globe s'était énormément allongé dans le sens normal au limbe solaire ou perpendiculaire à la première direction. Un petit amas de matière s'en était détaché à la partie inférieure et se trouvait suspendu entre le Soleil et le nuage principal. Le temps, qui se couvrit, ne me permet pas de poursuivre davantage.

"Resumons ces observations.

"Considérée d'abord dans son principe, la nouvelle méthode repose sur la différence des propriétés spectrales de la lumière des protubérances et de la photosphère. La lumière photosphérique, émanée de particules solides ou liquides incandescentes, est incomparablement plus puissante que celle des protubérances due à un rayonnement gazeux. Aussi a-t-il été jusqu'ici à-peu-près impossible d'apercevoir les protubérances en dehors des éclipses. Mais on peut renverser les termes de la question en s'adressant à l'analyse spectrale. En effet, la lumière solaire se distribue par l'analyse dans toute l'étendue du spectre et, par là, s'affaiblit beaucoup. Les protubérances, au contraire, ne fournissent qu'un petit nombre de faisceaux dont l'intensité reste très-comparable aux rayons solaires correspondants. C'est ainsi que les raies protubérantielles sont perçues très-facilement dans un champ spectral, sous le spectre solaire, tandis que les images directes des protubérances sont comme écrasées par la lumière éblouissante de la photosphère.

"Une circonstance fort heureuse pour la nouvelle méthode vient s'ajouter à ces données favorables. En effet, les raies lumineuses des protubérances correspondent à des raies obscures du spectre solaire. Il en résulte que non-seulement on les aperçoit plus facilement dans le champ spectral sur les bords du spectre solaire, mais qu'il est même possible de les voir dans l'intérieur de ce spectre et, par conséquent, de suivre la trace des protubérances sur le globe solaire même.

"Au point de vue de la détermination de l'espèce chimique, les procédés suivis pendant les éclipses totales comportaient toujours quelque incertitude: en l'absence de la lumière solaire, on était obligé de recourir à l'intermédiaire des échelles pour fixer la position des raies des protubérances. La nouvelle méthode permet de comparer directement les raies protubérantielles aux raies solaires. Les identifications sont alors absolument certaines.

"Au point de vue des résultats obtenus pendant la courte période où elle a été appliquée, la méthode spectro-protubérantielle a permis de constater:—

"1°. Que les protubérances lumineuses observées pendant les éclipses totales appartiennent incontestablement aux régions circumsolaires;

"2°. Que ces corps sont formés d'hydrogène incandescent, et que ce gaz y prédomine s'il n'en forme la composition exclusive;

"3°. Que ces corps circumsolaires sont le siége de mouvements dont aucun phénomène terrestre ne peut donner une idée; des

amas de matière dont le volume est plusieurs centaines de fois plus grand que celui de la Terre, se déplaçant et changeant complètement de forme dans l'espace de quelques minutes.

"Tels sont les principaux résultats obtenus. J'espère que malgré l'état de ma vue, fatiguée par mes longues études sur la lumière, je pourrai continuer ces travaux. J'aurai l'honneur de soumettre les résultats au Bureau des Longitudes.

"J'ajouterai en terminant que j'ai eu l'occasion de continuer aussi mes études sur le spectre de la vapeur d'eau. Le climat de l'Inde, très-humide en ce moment, est très-favorable à ces recherches. Je suis conduit à attribuer au spectre de cette vapeur une importance tous les jours plus grande, l'ensemble de mes études à Paris et ici me conduit à reconnaître une action élective sur l'ensemble des radiations solaires, depuis les rayons obscurs jusqu'aux rayons ultra-violets, bien que dans le violet l'action élective soit beaucoup plus difficile à constater. Ces études formeront l'objet d'une communication séparée."

Southern Survey.

It is with great pleasure that the Council bring before the Society the following report of Mr. Ellery's progress in this important work.

Progress Report of the Melbourne portion of the Southern Survey.

"The zone observations for the portion of the Survey allotted to the Melbourne Observatory were commenced on the 11th April, 1866, as I have already reported.

"It was determined at the outset not to accumulate observations beyond our powers of reduction, and the rule of having one month's work reduced before commencing another has been rigidly adhered to. With the limited staff of this Observatory, and the many other duties devolving on the officers, this course does not admit of very rapid progress being made.

"The work already done comprises the observation and final reduction to the arranged epoch, of all stars down to the 9½ and many to the 10th magnitudes.

Between parallels	150°·00	and	152°·40	N.P.D.	0h 0m to 24h 0m
,, ,,	152·40		153·20		0 10 22 10
,, ,,	153·20		154·00		11 40 15 40
,, ,,	154·00		154·40		12 10 13 30

"The number of stars observed in these zones and now reduced and tabulated is 20,471.

"The methods of observation have already been reported to the Society; our longer experience with them shows that they are well adapted to this kind of work. The two Chronographs, with

two hour hands and short conical pendulums, are admirably adapted to zone observations, and give a greater amount of accuracy and far less trouble than usually belongs to such observing. The glass reservoir pens used, however, were for a long time a great source of annoyance from their uncertainty of action, and a two-hours' zone was often spoilt by failures of the pen through plugging of the capillary bore of the nib, or from too free a flow. After many trials of different kinds of pens and pencils for recording, and of the various methods of marking the movements of the magnet armature on paper in the different forms of chronographs and telegraph instruments, I devised a pen which gives so much satisfaction in its operation, and appears so certain and reliable in its action, that I have sent a drawing and description of it as an appendix to this report.

"The mode of reading off the declinations by means of a micrometer dial and count-wheel, which indicate the position of the system of seven declination wires illuminated in a dark field, is, I am convinced, better than using a scale at the focus of the eyepiece. It admits of much more precision and relieves the observer immensely; all the reading-off from the micrometer being done by the assistant."

Spectroscopic Observations of the "Red Prominences" without an Eclipse of the Sun.

Our Fellow, Mr. Lockyer, communicated to the Royal Society, on October 11, 1866, his spectroscopic observations of solar spots, and some deductions from the appearances presented by the absorption lines. At the close of that paper Mr. Lockyer asks, "May not the spectroscope afford evidence of the existence of the red flames which total eclipses have revealed to us in the Sun's atmosphere, although they escape all other methods of observation at other times? and if so, may we not learn something from this of the recent outburst in *Corona?*" This prediction has since been most brilliantly fulfilled by the independent discoveries of M. Janssen and himself.

In January 1867, Mr. Lockyer made an application to the Government Grant Fund, administered by the Royal Society, for a more powerful spectroscope. The application was acceded to, and the construction of the instrument was intrusted to the hands of our late and much-lamented Fellow, Mr. Thomas Cooke. Increasing ill-health and engagement upon Mr. Newall's large object-glass, unfortunately, delayed its construction.

On January 7, 1868, the order was transferred to our Fellow, Mr. Browning, by whom it was finished, and placed in Mr. Lockyer's hands, October 16. At this time we were in possession of the results of the spectroscopic observations of the red flames made at the eclipse of August 18. That the red prominences consisted of luminous gas, giving a spectrum of bright

lines, was removed from the region of theory and conjecture to that of fact.

On the receipt of his spectroscope, Mr. Lockyer at once applied it to test whether it was sufficiently powerful to render the bright lines of the solar prominences visible by the comparatively greater dispersion of the surrounding light. His success was complete on the first day he gave to the work, viz. October 20th; he was enabled at once to determine that the prominences were built up of hydrogen by the absolute coincidence of two of the bright lines with the Fraunhofer's lines C and F. He was also enabled to establish a fact which may ultimately be of great importance, namely, that the bright line near Fraunhofer's D was not the line D itself, and that it did not absolutely coincide with any absorption line ordinarily present in the solar spectrum. Mr. Lockyer also was enabled to determine at once the shape and dimensions of the prominence he "felt" in his new instrument.

This discovery was announced to the Paris Academy by Mr. De La Rue, a few minutes after a second and more detailed communication from Mr. Lockyer had been placed in the hands of M. Delaunay, the President of that body, a letter was received by him from M. Janssen, who was then in India, announcing that he had also discovered the same method of research some time before, viz., on the day after the eclipse itself.

The number of cases of independent discoveries which have occurred in Science is most remarkable. It would be almost inexplicable were it not for our knowledge of the gradual steps by which each important discovery is led up to, and the number of active minds which are thus directed towards every question as it ripens for solution.

It may have been that had the unfortunate delay not occurred in the completion of his spectroscope, Mr. Lockyer would have been enabled to have anticipated the spectroscopic discoveries made during the eclipse of August 18, 1868. To leave, however, this region of conjecture, we must most warmly congratulate him on the important results which he has already obtained,—results which we hope may prove but an earnest of yet more important observations to follow.

Measures of ζ Herculis.

Mr. Knott has fortunately found a double star *ζ Herculis*, in which the companion falls within the first bright ring. He has thus been enabled to obtain, for the aperture of his telescope, the radius of the first bright ring of the star. The observed value is in most satisfactory accord with its value deduced, for mean rays, from the undulatory theory of light.

Motion of Sirius in the Line of Sight.

There are three absorption bands in the spectrum of *Sirius* which correspond very closely with the three characteristic lines of luminous hydrogen. Two of these bands have been directly compared with the spectrum of luminous hydrogen. The third is too faint for accurate comparison. The presence of all three characteristic lines and the close agreement of their positions with light of the refrangibilities of luminous hydrogen render it extremely probable that this gas is present in the absorbing atmosphere of the star. Mr. Huggins has, however, found that the brightest hydrogen line, or that corresponding to the solar line F, does not, with the dispersive power at his command, fall centrically within the corresponding absorption band in the spectrum of *Sirius*. The other lines are too faint for such delicate experimental inquiries. We cannot, however, doubt, for the reasons stated, that hydrogen is really present in the absorbing atmosphere of the star.

For comparison with the spectrum of the star, Mr. Huggins has employed hydrogen under different pressures. He has, however, shown that as the pressure is increased, the breadth of the corresponding band, as seen in his instrument, is symmetrically increased. The want of coincidence of the hydrogen line and the corresponding absorption line in the spectrum of *Sirius*, is not therefore a question of pressure. The coincidence of the three magnesium lines, *b*, with the corresponding absorption bands in the Moon's spectrum, appears in Mr. Huggins' apparatus perfect. This would appear to prove that the very great difficulty of insuring a similar refraction through the prisms for the beam from the near terrestrial light and the distant object whose spectrum is under examination, has been successfully overcome.

The want of absolute coincidence of the hydrogen line and the centre of the absorption band in the spectrum of *Sirius*, has been attributed by Mr. Huggins to a change in the refrangibility, of the light arising from the relative radial motions of the Earth and the star. Mr. Huggins' measures have assigned to *Sirius* a velocity of 29·4 miles a second receding in the direction of the line of sight.

Observations of the Spectra of Southern Nebulæ.

Lieut. John Herschel has successfully applied the instruments placed in his hand by the Royal Society for the observation of the Solar Eclipse of August last to an examination of the spectra of the Southern Nebulæ. It is interesting to find that the objects examined by him give, in some cases, a continuous spectrum, and in others a spectrum of one or more of the same bright lines which are seen in the Nebulæ which had been observed in England.

In the Great Nebula in *Orion* he has detected, in addition to the three lines, a fourth very faint line, more refrangible than F, and probably in the same position as the fourth line seen by Mr. Huggins in 18 H, IV. It is probable that this line may coincide with a line of hydrogen. Lieut. Herschel has also detected a faint continuous spectrum. Lord Rosse and Prof. Winlock have made similar observations, and suspect in addition the presence of some other lines.

The Nebula in Orion.

Two valuable drawings of the Great Nebula in the Sword-hilt of *Orion* have been communicated to the Royal Society, and a third has been published and distributed to Astronomers by Padre Secchi. The most comprehensive of these pictures is that contributed to the Royal Society by the Earl of Rosse, together with a paper, which have been recently published in the *Phil. Trans.* vol. clviii. 1868. The representation of the Nebula is $23\frac{1}{2}$ in. × $25\frac{1}{2}$ in., and comprises about one square degree, namely, 20′ to the north and 40′ to the south of the principal star in the trapezium; and 26′ to the east and 38′ to the west of that star; the nebulous matter extending beyond the star *iota*. This very beautiful picture is the graphic summation of seven years' work, performed with admirable zeal and continuity of purpose by the late noble founder and constructor of the Birr Castle telescopes, and of his staff of assistants; and also that of the present Earl, who has completed the observations commenced by his father. During these seven years it was found necessary to devote the 6-foot and 3-foot reflectors, and occasionally an 18-in. Cassegrainian, mounted equatorially, on every available hour to the work in hand, in order to produce the picture which has been published by the Royal Society. It is printed in fac-simile of the original in such a way that black represents white, but the Council have wisely had printed for distribution a certain number of copies in white ink on a black paper, thus representing the object as in nature.

The second drawing, in which black also represents white, was presented to the Royal Society by Mr. Lassell, in April 1868; it sums up the results of much arduous work performed with his 4-foot Newtonian telescope mounted equatorially, during his sojourn in Malta, extending over a period of rather over three years. This drawing is 25 inches square; it comprises a space of 13′ 20″ to the north and 15′ to the south of the principal star in the trapezium, also 15′ to the east and 13′ 20″ to the west of that star; consequently not quite $\frac{1}{4}$ of a square degree (0·223).

Astronomers have thus at command two drawings of this object by different observers, but made with the aid of instruments comparable as to aperture, although not of precisely similar dimensions. A cursory glance impresses one with the general similarity of the appearances depicted, but a closer inspection of the two pictures renders evident a great disagreement in many points of detail, quite great enough indeed to mislead in respect to any con-

clusion which might be drawn as to changes having taken place in the configuration of the nebulous matter. This is not to be wondered at, for this object varies so considerably in its aspect not only with instruments of different apertures, but also with the same instrument under different conditions of atmosphere, that it is almost impossible to state with any degree of certainty whether any physical changes have taken place in the several parts of the Nebula during the period in which it has been carefully scrutinised. The importance to future Astronomers of having drawings which have been made at about the same epoch by well-practised observers, with the aid of competent and very similar instruments, cannot therefore be overrated; and it is to be hoped on this account that the Royal Society will publish Mr. Lassell's drawing.

Padre Secchi's picture represents a space comprised within the limits of 10′ to the north, 12′ to the south, 10′ to the east, and 12′ to the west of the principal star of the trapezium; or 0·134 square degree; it is also printed in black for white. On taking into consideration the comparatively very small aperture of the instrument employed it exhibits a great amount of detail; and will have its value in the future, more especially as presenting some such general aspect of this nebula as the valuable picture which a finder to large telescope offers of a comet to be scrutinised for minute details in its larger companion. It differs, however, as might be expected very considerably from both Lord Rosse's and Mr. Lassell's drawings.

It is to be expected that now that the Melbourne 4-foot Cassegrainian has arrived at its destination, and is being erected, another record will be made of the aspect of this vast assemblage of nebulous and stellar matter, under the peculiarly favourable circumstances which the handiness of the instrument and the advantageous position of the observing station present.

When one reflects on the vast dimensions of the Nebula of *Orion*, it is difficult to realise the probability of changes occurring to such an extent in the distribution of the nebulous matter as to be recognisable at very short intervals; it is, therefore, as a basis for comparison with observations made by Astronomers some centuries hence, that the pictures to which we have drawn attention must be regarded as of the highest value; for in these remote periods possibly real changes will have occurred which cannot be confounded with the apparent changes dependent on variations of atmospheric conditions.

The Variable Star η Argûs and its Surrounding Nebula.

The papers which Mr. Abbott has communicated to the Society on this subject appear to indicate changes in the distribution of the nebulous matter around *η Argûs* of the most rapid and astounding character.

Sir John Herschel, in the *Notice* for June, vol. xxviii., has pointed out his inability to identify any of the stars near *η Argûs*, in Mr. Abbott's drawing, and expressed the strongest wish that the point thus raised of the reality of the changes in the Nebula around *η Argûs* should be settled definitely and at once. The wish thus expressed was not likely to remain disregarded.

A drawing of the Nebula has already been presented to the Society by Lieut. Herschel. In this drawing the star *η Argûs* is represented as quite within the region of nebulous matter. Lieut. Herschel's drawing agrees far more closely with his father's drawing of 1834–7 than with either of Mr. Abbott's drawings. It is, however, desirable that the Nebula should be examined under more favourable circumstances than those at the command of Lieut. Herschel.

November Meteors.

The shower of last November assumed almost unexpected proportions. Unfavourable weather interfered to a considerable extent with its observation in England. A very considerable number of meteors were, however, observed, and the position of the radiant fixed with accuracy at the Greenwich Observatory. Observers in the south of Europe, at the Cape of Good Hope, in Australia, and on the American continent, were more fortunate. The mass of materials thus collected proves the shower to have been on a most extensive scale. The succession of large November showers for at least four successive years, would appear to prove that the arc of the orbit over which considerable condensation exists is far more extensive than was formerly supposed.

The Approaching Transits of Venus of 1874 *and* 1882.

The Society, and all interested in the advancement of our noble science, are indebted to the Astronomer Royal for an elaborate discussion of the best means for utilising the approaching Transits of *Venus* in 1874 and 1882.

The method of differences of observed durations will not be the best available for the determination of Solar Parallax in 1874, and probably also not in that of 1882.

It will thus be necessary to determine with extreme accuracy the longitudes of the different stations. This necessity calls for immediate action. The Astronomer Royal has most carefully pointed out in his paper the stations which are best adapted for the location of the observers. It is one of the many advantages which we may expect to derive incidentally from the proposed expeditions, that we shall be thus driven to determine with extreme accuracy the position of so many distant stations, which will afterwards be available as zeros of longitude.

Mr. Warren De La Rue has urged the advisability of supplementing the eye-observations by photography. He is of opin-

ion that a value of the solar parallax of considerable weight may be thus obtained. His great experience and proved skill in the deduction of accurate results from photographic registers entitle his opinion to the greatest ocnsideration.

The Astronomer Royal has proposed that altazimuths should be employed in the determination of the longitudes of the stations. Results of very much the same value as those to be expected from photography might probably be obtained without any additional expense, or interfering in the slightest degree with any other results by simply observing throughout the transits, with those altazimuths, the position of the planet with reference to the Sun's limbs.

The Astronomer Royal's application for the necessary funds is sure to meet with that liberal consideration which the British Government has ever shown towards all well-considered schemes for the advancement of science.

Discovery of Minor Planets.

Twelve Minor Planets have been discovered during the year 1868. This is the greatest number added to the list of planets in any single year. It will be seen from the tabular statement below that the diligence and zeal of Professor Watson of Ann Arbor, Michigan, have been rewarded by the discovery of no less than six out of the twelve of these minute members of the solar system. The date of each discovery is as follows:—

Planet.	Discoverer.	Date of Discovery.
Ægle	Coggia	1868, February 17
Clotho	Tempel	,, February 17
Ianthe	C. H. F. Peters	,, April 18
(99)	Borelly	,, May 29
Hecate	Watson	,, July 11
Helena	Watson	,, August 16
Miriam	C. H. F. Peters	,, August 22
(103)	Watson	,, September 7
(104)	Watson	,, September 13
(105)	Watson	,, September 16
(106)	Watson	,, October 10
Camilla	Pogson	,, November 17

A Comet, Comet I. 1868, was discovered by Dr. Winnecke on June 13.

Brorsen's and Encke's periodical Comets have been observed at their respective oppositions.

Spectrum of Comet II. 1868.

The spectrum of this faint Comet has been examined by Mr. Huggins.

There appears a very close agreement between its spectrum and that of the vapour of carbon, when obtained by the decomposition of olefiant gas and some other compounds of carbon.

Rediscussion of the Observations of the Transit of Venus, 1769.

Our Secretary, Mr. Stone, has done an important service for Astronomy by an independent discussion of the observations of the Transit of *Venus*, 1769. Skilfully applying to the descriptions of the observers an interpretation founded on the optical phenomena which necessarily attend the transit of a small dark body over the Sun, he has shown that the observations of the different observers, so far from being discordant, are in close agreement with each other, and furnish a value of the Solar Parallax which is almost identical with the new value which results from the observations of *Mars*. A more complete account of this investigation, and of Mr. Stone's other contributions to our science, will be found in the President's statement of the reasons for the award by the Council of the Gold Medal to that gentleman.

Reference in this place should be made, however, to some other papers which Mr. Stone has presented to the Society during the present year. One of these contains an investigation of the principles on which observations, which differ considerably from the general average, should be dealt with. He has deduced therefrom for different values of the average accuracy of the observer, and with certain assumed probable errors, the limits beyond which all discordant observations should be rejected.

In another paper, from an independent determination, he has found for the Moon's mass the value—

$$\frac{\text{Moon's Mass}}{\text{Earth's Mass}} = \frac{1}{81{\cdot}36}$$

Two papers are devoted to a determination of the constant of nutation, from observations in N.P.D. of *Polaris*, *Cephei* 51, and δ *Ursæ Minoris*, and in R.A. of *Polaris*, made at Greenwich from 1851 to 1867.

Communications to the Society from February 1868 *to February* 1869.

1868.

Mar. 13. Additional Identification of Double Stars in Sir William Herschel's Catalogue. Sir J. Herschel.
Observations of Minor Planets. Dr. R. Luther.
On the Lunar Crater *Linné*. Rev. T. W. Webb.
Sur les Comètes, 1857, III. and V., and 1867, III. M. Hoek.
On the Planetary Nebula 45 ♅ IV. *Geminorum*. Rev. H. C. Key.
Sur les Phénomènes qu'un essaim de Météores très étendu venant de l'Espace présente après son entrée dans le Système Solaire. M. Hoek.

April 8. On the Lunar Crater *Linné*. Mr. Crossley.
On some Markings seen on *Venus*. Mr. Browning.
Catalogue of Known and Suspected Binary Stars. Mr. Chambers.
On the Employment of the Table given in the *Nautical Almanac* showing the correction required on account of second differences in finding the Greenwich Time corresponding to a reduced Lunar Distance. Capt. Shadwell.
On the Lustre of *Venus*. Mr. Weston.
Description of the Zenith Telescope of the United States Survey. Mr. Davison.
On the Rejection of Discordant Observations. Mr. Stone.
The Great Nebula of *Orion*. Father Secchi.
Proposal for a New Star Atlas. Mr. Proctor.
Lunar Crater *Linné*. Capt. Noble.

May 8. Occultation of γ *Tauri*. Mr. Joynson.
Lunar Crater *Linné*. Rev. T. W. Webb.
On the Variability of η *Argûs* and the surrounding Nebula. Mr. Abbott.
Eclipses of *Jupiter's* Satellites. Mr. Tebbutt, jun.
On the Stellar Longitudes assigned by Ptolemy. Major Drayson.
On the Lunar Crater *Linné*. Mr. Prince.
Remarks on the Name of the Star ξ *Scorpii* (= Fl. 51 *Libræ*). Mr. Brothers.
On a new Driving Clock for Equatoreals. Mr. Cooke.
Ephemeris of Brorsen's Comet. Dr. Bruhns.
Questions relative to the Invention of the Achromatic Telescope. Mr. Wackerbarth.
Note on *Linné*. Mr. Birt.
Catalogue of Spectra of New Stars. Father Secchi.

List of Stars to be Occulted about the time of the Great Eclipse, August, 1868. M. Ragonatha Chary.

On a Persistent Marking on *Jupiter*. Mr. Browning.

June 12. On the Variable Star η *Argûs* and the surrounding Nebula. Sir J. Herschel.

Observations of the Lunar Crater IV. A. α 17 IV. A. 339. Mr. Birt.

Determination of the Constant of Nutation from the Observations in N.P.D. of *Polaris*, *Cephei* 51 and δ *Ursæ Minoris*, made with the Greenwich Transit Circle, 1851 to 1865. Mr. Stone.

Nov. 13. On the Nebular Hypothesis and the Approximate Commensurability of the Planetary Periods. Mr. Kirkwood.

Re-discussion of the Observations of the Transit of *Venus*, 1769. Mr. Stone.

Observations of the Total Eclipse of August 18, 1868. Mr. Reed.

Ditto ditto Mr. Tebbutt, jun.

Observations of the Transit of *Mercury*, November 4, 1868. Prof. Chevallier.

Ditto ditto Mr. G. Williams.

On the Discovery of the Variable Star T *Coronæ*. Mr. Barker.

Description of an Automatic Transit Instrument. Mr. Kincaid.

Occultations observed at Durham. Prof. Chevallier.

On the Early History of the Achromatic Telescope. Mr. Chambers.

Transit of *Mercury*. Mr. Prince.

Ditto ditto Prof. C. P. Smyth.

Description of Drawings of *Mars*. Mr. Joynson.

Occultations of Stars of *Jupiter's* Satellites. Mr. Joynson.

Heliographical Elements of Sun-spots, August 18, 1868. Mr. Loewy.

Transit of *Mercury*. Mr. Lassell.

Field Notes of Zenith Telescope. Mr. Davidson.

On the Appearance of *Mercury* at its Transit, Nov. 5, 1868. Mr. Huggins.

On a possible Method of Viewing the Red Flames without an Eclipse. Mr. Huggins.

On the Lunar Crater *Linné*. Mr. Birt.

Letter to Rev. C. Pritchard. Sir J. Herschel.

Observations at Greenwich of the Egress of the Planet *Mercury* from the Sun's Disk, Nov. 5, 1868. The Astronomer Royal.

On a proposed Form of Spirit Level. Major Tennant.

Remarks on Mr. Stone's Paper on the Transit of *Venus*, 1769. Mr. Newcomb.

Reply to on Mr. Newcomb's Remarks on his Paper on the Transit of *Venus*, 1769. Mr. Stone.
Some Remarks and Suggestions arising from the Observations of the Transit of *Mercury*, 1868, Nov. 4. Mr. Stone.
Determination of the Constant of Nutation from Right Ascension Observations of *Polaris*. Mr. Stone.
On Aboul Hassan's Catalogue of Stars. Mr. Stone.
The anticipated Weather of 1869. Mr. Best.
On the Cause of the Difference of Time between the Mornings and the Evenings. Mr. Best.
Transit of *Mercury*. Mr. Penrose.
Ditto ditto Capt. Noble.
Ditto ditto (Photographs). Rev. Dr. Selwyn.
Reports on Solar Eclipse, Aug. 1868. Mr. Smith.
Note on his Catalogue of Binary Stars. Mr. Chambers.
Description of a new Chronograph Pen. Mr. Ellery.
Transit of *Mercury*. Mr. Buckingham.
Dec. 11. Meteors of November 1868. Mr. Kirkwood.
Measures of ζ *Herculis*. Mr. Knott.
Transit of *Mercury*. Mr. Knott.
Ditto ditto Mr. Browning.
Further Paper on an Automatic Transit Instrument. Mr. Kincaid.
Elements of newly discovered Asteroids. Mr. Watson.
Observations of the Solar Eclipse, Aug. 1868. M. Cheetry.
Ditto ditto ditto Maj. Tennant.
On a Defective Vision. Major Tennant.
Ephemeris for Observations of the Physical Features of the Planet *Mars* during the Opposition of 1869. Mr. Marth.
Observations of the Meteoric Shower of Nov. 13–14, 1868. Prof. Grant.
Preparations for Observing the Transits of *Venus*, 1874, 1882. The Astronomer Royal.
On the Observations of the Transits of *Venus* by Photography. Mr. De La Rue.
Observations of Meteors, 1868, Nov. 14. Admiral Sir E. Belcher.
1869. Transit of *Mercury*. Mr. Talmage.
Jan. 8. On the Physical Constitution of the Sun. Commander Ashe.
Comparison of Sun-spot Observations made in Dessau and Kew Observatory. Messrs. De La Rue, Stewart, and Loewy.
On the Relation of the Luminous Prominences to the Faculæ of the Sun. Mr. Brayley.
Transit of *Mercury*. Mr. Todd.
Description of the Great Nebula round η *Argûs*. Sir J. Herschel.

Note on Mr. Huggins' Paper, "On a Possible Method of Viewing the Red Flames without an Eclipse." Mr. Lockyer.
Meteors of Dec. 11, 1868. Mr. Finlayson.
Letter to Admiral Manners. Sir J. Herschel.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
The Lords Commissioners of the Admiralty.
The Secretary of State for War.
Royal Society of London.
Royal Asiatic Society.
Royal Geographical Society.
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ADDRESS

Delivered by the President, Admiral Manners, on Presenting the Gold Medal of the Society to Mr. E. J. Stone.

Gentlemen, — The pleasing duty now devolves upon me, as President of this Society, to give effect to the resolution which you have adopted with such cordial unanimity by presenting to Mr. Edward James Stone the usual form of testimonial, which, in accordance with the bye-laws, is awarded annually by the Society as a recognition on its part of special services rendered in promoting the science of Astronomy.

Mr. Stone, as many of you are aware, has for nearly nine years occupied the responsible position of First Assistant at the Royal Observatory, Greenwich. He succeeded in this post to a gentleman, a valued member of our Society, who for many years had discharged the duties of his office in a manner at once eminently honourable to himself and advantageous to the interests of the renowned Institution with which he was associated. I need scarcely inform you that I refer to the able Director of the Radcliffe Observatory, Oxford.

Educated at the University within the ancient Halls of which Newton completed his academical studies, Mr. Stone entered upon his duties at the Royal Observatory with a mind thoroughly trained in a course of instruction which was eminently adapted to the career in life he had chosen.

I have reason to believe that, during the period of his connexion with the Royal Observatory, Mr. Stone has conducted himself throughout in such a manner as to earn the unqualified approbation of the Chief of that Establishment. With this, however, we have nothing to do on the present occasion.

The Society, except under circumstances of very rare occurrence, takes no cognizance of labours, however honourable and useful, which may have been performed merely in the ordinary course of official life.

We are now met, Gentlemen, to affix the seal of our recognition to services which have been rendered to Astronomy during leisure hours exclusively, which have been prompted solely by that ardent thirst for knowledge and that irrepressible longing after truth which constitute one of the distinguishing features of our common humanity.

I shall now proceed to state to you, in as concise terms as circumstances will admit of, the grounds upon which the Council unanimously resolved that the Medal of the Society should be awarded this year to Mr. Stone.

During the last few years Mr. Stone has contributed to the Society a great number of important papers on different subjects of Astronomy, which have been published either in the *Memoirs* or in the *Monthly Notices* of the Society.

Any person who gives those papers an attentive perusal will not fail to discover that they have for their leading object the investigation of improved values of the fundamental constants of astronomy by a searching discussion of large masses of trustworthy observations.

In the prosecution of his researches Mr. Stone has availed himself chiefly of the stores of valuable observations which have accumulated at the Royal Observatory, Greenwich, more especially during the incumbency of the present illustrious Director of the Institution.

I shall commence my brief review of Mr. Stone's astronomical labours with some account of his researches on the Solar Parallax.

The ancient astronomers were absolutely unacquainted with even an approximate value of the Solar Parallax. Their observations were of too rudimentary a character to exhibit any trace of an element of such delicate minuteness. The same ignorance prevailed in modern times till towards the close of the seventeenth century, when the application of the telescope to divided instruments and the invention of methods for ascertaining the errors of adjustment of instruments placed in the meridian, resulted in effecting a complete revolution in practical astronomy.

It was then only that the results of astronomical observation

acquired such a degree of precision as to hold out any hope of detecting a change in the apparent position of the Sun, or any of the planets, depending upon the inclination of the line of vision of the object with respect to the imaginary line connecting the observer with the centre of the Earth. It is well known that in the case of the Sun the determination of such a change of apparent position by direct observations of that body is beset in practice with insurmountable difficulties. Fortunately the planets *Venus* and *Mars*, which revolve, the one immediately within, the other immediately beyond the Earth's orbit, supply the astronomer with excellent methods for attacking the problem of the Solar Parallax by an indirect course of procedure.

I shall first refer to the case of *Mars*.

The orbit of this planet, besides being very excentric, is so situated with respect to the Earth's orbit that the perihelion of the one is turned very nearly towards the aphelion of the other. The result is that, in the course of successive intervals of seven or eight synodic revolutions, the planet when at the perihelion approaches very near to the Earth, and, in consequence, occupies a position which is eminently favourable for the investigation of its parallax.

We owe to Dominique Cassini the earliest determination of the Solar Parallax by a method based upon sound principles.

By a comparison of meridian observations of *Mars* made in 1671 by Richer, at Cayenne, in South America, with corresponding observations of the planet made in Paris, he obtained $9''{\cdot}5$ as the most probable value of the Solar Parallax.

This was an immense step in advance, for, in all previous investigations the Solar Parallax was estimated in minutes. Several subsequent attempts to determine the Solar Parallax were made at different times down to the middle of the last century on the occasion of the near approach of *Mars* to the Earth, but in hardly any instance was the result so good as that which Cassini originally deduced from his researches on the subject.

The method for determining the Solar Parallax to which I have just referred was now about to be supplanted for a time by a new method founded upon observations of the transits of *Venus* over the Sun's disk. The peculiar advantages of this method had been forcibly pointed out by Halley.

The transits of 1761 and 1769 were close at hand, and the astronomers of the day were so far successful in impressing the public mind with their supreme importance, that the principal Governments of Europe sent out expeditions to different parts of the world for the purpose of making accurate observations of them. The results of those expeditions are well known. The transit of 1761 was from various causes imperfectly observed. The operations for observing the transit of 1769 were, on the other hand, decidedly successful. Shortly afterwards, various results for the Solar Parallax were obtained from the observations,

fluctuating between 8″·6 and 8″·8; but, as no method of discussion by a comprehensive and systematic process had been employed by any astronomer in such investigations, the definitive result of the observations could not be said to have been determined. This object, however, was finally effected by Encke, in his treatise, "Der Venusdurchgang von 1769," published at Gotha in 1824. The celebrated German astronomer determined the Solar Parallax to be 8″·5776 as the definitive result of his researches; and this value has since been universally adopted by astronomers till within the last few years.

In 1836, Professor Henderson determined the value of the Solar Parallax by a comparison of a few observations of *Mars* made at the Cape of Good Hope in 1832, with the corresponding observations made in the northern hemisphere at Greenwich, Cambridge, and Altona.

He obtained 9″·028 as the result of his researches, but the materials of his investigation were too scanty, and the residual errors were too discordant to affect the trustworthiness of the result which Encke had already deduced from the transit of *Venus* in 1769.

At the meeting of this Society, held in May 1857, the Astronomer Royal directed the attention of the Society to the great importance of determining the Solar Parallax by means of observations of *Mars* made at the approaching oppositions of 1860 and 1862.

This may now be the proper place to state that suspicions had already begun to be entertained respecting the accuracy of the value of the Solar Parallax which Encke had deduced from the transit of *Venus* in 1769.

The earliest indication of the necessity of an alteration of the received value is to be found in a letter from Professor Hansen to the Astronomer Royal, which is published in the *Monthly Notices* for November 1854 (vol. xv.). In this letter Professor Hansen states that the coefficient of the parallactic equation in the Moon's longitude, as determined by the Greenwich Observations, necessarily implies a greater value of the Solar Parallax than that which had been deduced from the transit of *Venus*, and he adds that the Dorpat Observations led to a similar conclusion.

The results of M. Le Verrier's researches shortly afterwards indicated the necessity of a similar increase of Encke's value of the Solar Parallax. In the *Annales de l'Observatoire Impérial de Paris*, vol. iv. 1858, he has determined from observation the coefficient of the lunar inequality in the Earth's longitude, and hence obtains 8″·95 for the value of the Solar Parallax. This result he has incorporated in his Solar Tables.

In vol. vi. of the same work the researches of M. Le Verrier on the movements of *Mars* and *Venus* conduct him to a similar conclusion as regards the necessity of increasing the received value of the Solar Parallax.

It may be mentioned also that about the same time the late Mons. Foucault obtained, by an ingenious arrangement, a measure of the velocity of light which necessarily implied a diminution of the radius of the Earth's orbit.

Such, then, Gentlemen, was the state of the question of the Solar Parallax previous to the opposition of *Mars* in 1862.

The opposition of 1860 had passed without attracting any special notice, the attention of astronomers having been mainly occupied with the total eclipse of the Sun, which happened in the month of July of that year. Fortunately, on the other hand, the opposition of 1862 was well observed at several important stations both in the northern and the southern hemispheres.

The principle of the method which was generally agreed upon as best adapted for the determination of the Solar Parallax in this instance, consisted in observing with meridional instruments the differences of declination between the planet and certain comparison stars which lay in the vicinity of its apparent path. Upon the basis of a considerable portion of the materials thus formed, Mr. Stone investigated the Solar Parallax in a paper which has been published in the *Memoirs* of the Society.

I cannot do better than quote his words with reference to the advantages which the method offers for effecting the object in view: — "The peculiar advantages of the method adopted appear to be that the observations can be made with the instruments of fixed observatories by observers in the ordinary routine of their work, the results obtained are so far removed from the quantities immediately observed that no knowledge of the results expected to be obtained can influence the observers. The number of observers can be sufficiently multiplied to diminish considerably every injurious effect of personality, by taking stars properly distributed above and below, and preceding and following *Mars* the effects of errors of the refraction-correction, at least in a mean result, can be destroyed; the effects of any division errrors likely to exist uncorrected in our modern instruments can be destroyed in the same way; and lastly, the observations may be indefinitely multiplied, and a continuous approximation carried on by availing ourselves of the favourable oppositions of *Mars*."

The materials which constitute the groundwork of Mr. Stone's investigation consist of observations of *Mars* and neighbouring stars made at Greenwich, the Cape of Good Hope, and Williamstown in Victoria, during the interval included between 1862, August 25, and November 23.

The results of his researches are exhibited in a system of nine tables. The three first tables contain the reduced north polar distances of the planet as determined at the three observatories. In the fourth and fifth tables the observations of the comparison stars made at the Cape of Good Hope and Williamstown are compared with the corresponding Greenwich errors and appropriate corrections hence determined.

In the sixth table the error of the Ephemeris of the planet is investigated by the aid of the Greenwich Observations.

In the seventh table the Greenwich and Cape Observations are combined together, and the Sun's equatorial horizontal parallax computed for each separate day of observation, each result having a certain weight attached to it.

In the eighth table the Greenwich and Williamstown observations are similarly combined together, and the resulting parallaxes exhibited with their appropriate weights.

In the ninth and last table the parallax for each day of observation, with its proper weight attached to it, is computed from a combination of the Greenwich, Cape of Good Hope, and Williamstown Observations, and the definitive value of the Solar Parallax is deduced. This Mr. Stone finds to be $8''\cdot943$, with a probable error of $0''\cdot051$.

It is unnecessary for me to expatiate upon the merits of this excellent paper.

The resulting value of the Solar Parallax obtained by Mr. Stone on this occasion presented a satisfactory agreement with the values of the same element which had been recently derived from other sources, and tended to strengthen the suspicion already entertained respecting the result which Encke had deduced from the observations of the Transits of *Venus*.

The next determination of the Solar Parallax by Mr. Stone is contained in a paper on the "Value of the Co-efficient of the Parallactic Inequality in the Moon's Longitude, determined by means of the Observations of the Moon made at Greenwich between the years 1848 and 1866."

The materials of this discussion consist of 2075 observations of the Moon.

The resulting value of the co-efficient of the parallactic equation is found to be $125''\cdot36$, and hence the value of the Solar Parallax is determined to be $8''\cdot850$.

I should mention here that Professor Hansen, in a communication to the Society which appears in vol. xxiv. of the *Monthly Notices*, had assigned $8''\cdot916$ as the value of the Solar Parallax derivable from his researhes on the lunar theory.

In 1864 Mr. Powalky subjected the observations of the transits of *Venus* to a fresh discussion, and determined the Solar Parallax to be $8''\cdot832$.

This discussion, which is evidently the result of very much labour, is based upon more accurate determinations of longitude than those available in Encke's time.

The rejection of many observations at the more important stations for reasons which do not appear to be warranted by any legitimate principles, and the imperfect manner in which the very few durations employed are represented in the residuals detract much from the value which would otherwise attach to this result. The weight of Encke's value would scarcely be said to be affected by this discussion.

That value still continued to put a veto on any acceptance of the values which resulted from the other methods which had applied to the determination of the Solar Parallax.

Mr. Stone's rediscussion of the Transit of *Venus* of 1769 is published in the Supplementary number of the twenty-eighth volume of the *Monthly Notices*. The materials of his researches consist of ten complete observations of the Transit made at Wardhus, Hudson's Bay, Kola, St. Joseph in California, and Otaheite.

Incomplete observations were excluded in consequence of the uncertainty which in general existed respecting the longitudes of the several stations, and the sufficiency of the observed durations for an accurate determination of the parallax.

Mr. Stone uses both apparent and real internal contacts. Corresponding to each phase, or rather to each mean phase of apparent contact and real contact, he introduces an unknown quantity. These unknown quantities are determined with the parallax correction from the ten equations of condition. The two corrections corresponding to apparent and mean phase are not, however, independent; their difference must be equal to twice the time between the mean phase of apparent and real contacts.

The resulting value of Solar Parallax is $8''{\cdot}91$, with a probable error of about $0''{\cdot}03$.

This result presents a most satisfactory agreement with the results of recent researches on the same subject derived from other sources.

Furthermore, the differences between the observed and computed durations are reduced unequivocally within the limit of the errors of observation. Thus, for example, in the case of the observations of Father Hell at Wardhus, which proved such a stumbling-block in previous researches, the residual error is reduced to $1^s{\cdot}7$. In point of fact the outstanding discordance between the observed and computed duration is in every case below 6^s.

Now, with reference to this investigation of Mr. Stone's, I would remark that it includes every complete observation from internal contacts of the transit, and yet, notwithstanding this circumstance, all the observed durations are represented with a degree of accuracy which is, beyond all doubt, within the limit of the probable errors of observation. The author has effected this object by simple interpretation of the words of the observer, inferring from his language and from the accompanying circumstances of the case whether the phenomenon noted by him referred to an apparent contact or a real contact.

This is all the liberty that Mr. Stone has allowed himself, and even this liberty has not been available except in those cases where the observer's own words are ambiguous. He has rejected nothing.

It is true that the method adopted by Mr. Stone involves the introduction into his researches of an unknown quantity corresponding to each observed phase. This is, however, equivalent to the arbitrary selection of real contact observations which had

always been previously done. In Mr. Stone's investigation, as I have already stated, the two corrections thus introduced are not independent but subject to a necessary condition. This condition is satisfied. There could not be a stronger proof than this of the legitimacy of the method pursued by Mr. Stone, according to which he introduced into his researches apparent or real contacts indiscriminately, according as the one or the other was noted by the observer.

In his paper on this subject Mr. Stone has given a faithful citation of the original observations, by which any person may satisfy himself respecting the interpretation which the author has put upon the words employed by each observer.

I have carefully perused those records, and I feel bound to state that, taking due note of the effects of irradiation and of the distinction existing between apparent and real contacts, the interpretation put upon the words of the original by Mr. Stone appears to me to be their true and simple meaning, and to be therefore the only legitimate interpretation which they can admit of.

By this important investigation, then, Mr. Stone has earned for himself the gratitude of Astronomers of all countries. He has shown beyond all doubt that the method pursued by his illustrious countryman Halley, when fairly treated, is capable of furnishing a value of the Solar Parallax commensurate in precision with the expectations formed of it.

But this is not all. Mr. Stone, by his researches in this instance, has wiped from Astronomy a reproach, which did not indeed legitimately attach to it, but which only one of those intellectual triumphs which from time to time have adorned the annals of our science was capable of extirpating.

I would mention further that by thus showing how completely the resulting value of the Solar Parallax depends upon apparently so slight a cause as misconceptions of obvious words respecting real and apparent contacts, he has done invaluable service in preparing the minds of Astronomers for the proper observation of the approaching transits of 1874 and 1882, and saved Astronomy from the possibility of a compromising and expensive failure.

We owe to Mr. Stone an important paper on the Constant of Lunar Parallax. His investigation is founded on a comparison of north polar distances of the Moon, determined at Greenwich during the years 1856–61, with corresponding places deduced from observations made at the Cape of Good Hope.

The aggregate number of observations is 239. As it is well known that considerable personality exists in the measurement of the disks of the various bodies of the planetary system, the author adopted the expedient of discussing separately the north and south limbs of the Moon.

The results to which he was conducted by this mode of procedure fully justified the considerations by which he was guided. He ascertained, in fact, that the correction to the constant of lunar parallax resulting from observations of the two limbs was sensibly

different. By a judicious combination of the separate results, however, he obtained a definitive correction, which, when applied to the theoretical value of the constant, conducted him to a value represented by 3422″·707, with a probable error of 0″·049.

The amount of the correction deduced in this instance was small, but the necessity of its application was plainly indicated by the observations, and it presents a perfect agreement with the result of a similar investigation due to Mr. Breen which is published in vol. xxxii. of the *Memoirs of the Society*.

I have now to make a few remarks on certain researches by Mr. Stone bearing more directly upon the accurate determination of the places of the stars. I would refer in the first place to a paper by him "On the Accuracy of the Fundamental Right Ascensions of the Greenwich Seven-year Catalogue for 1860."

It is well known that during the incumbency of the present Director there have emanated from the Royal Observatory, Greenwich, three Star Catalogues of fundamental importance.

First, we have the Twelve-year Catalogue of 2156 Stars, epoch 1845; then we have the Six-year Catalogue of 1576 Stars, epoch 1850; and, finally, we have the Seven-year Catalogue of 2022 Stars, epoch 1860.

Now a consideration of these Catalogues suggests two questions which it is most desirable to elucidate. In the first place, are there any grounds for supposing that the ultimate results may be affected by systematic errors, whether arising from irregularities in the rate of the clock or in the instrumental corrections, which the methods of reduction actually pursued have been incapable of eliminating?

And, secondly, is the process of reduction and successive cataloguing such as to lead to progressive accuracy as regards the resulting star-places?

Now, with reference to these points, Mr. Stone remarks that the method employed at Greenwich for determining the mean right ascensions of the fundamental stars has a tendency to affect the results with a common error, the elimination of which may be effected by observations of the Sun at the equinoxes. But what must be especially guarded against is the introduction of systematic errors, which neither observations of the Sun nor a repetition of the observations of the stars themselves can have the slightest tendency to eradicate.

Mr. Stone's method of investigation is accordingly based upon a separation of the assumed right ascension of the stars from any systematic errors with which they might be affected in the process of reduction. With a view to this object, he selects four stars of reference situated at nearly six hours of right ascension apart from each other, *γ Pegasi*, *Pollux*, *Spica*, and *α Aquilæ*, and then assuming the mean right ascension of those stars as given in the Seven-year Catalogue with unknown corrections applied to them, he forms a system of equations involving the *relative* corrections of the mean right ascensions of every combination of the reference

stars, taken two and two, for each of the seven years upon which the Catalogue is based, the relative corrections for each pair of stars being determined by means of the intermediate fundamental stars.

Finally, by the combination of the equations for the different years, he determines the definitive value of the *relative* corrections of the mean right ascensions of the four representative stars.

The result of a comparison of the relative corrections is highly satisfactory, inasmuch as it shows clearly the absence of any systematic errors of appreciable magnitude depending upon the right ascensions of the stars. Mr. Stone shows, furthermore, in the same paper, that the outstanding relative corrections of the Seven-year and Six-year Catalogues are considerably smaller than the relative corrections of the Seven-year and Twelve-year Catalogues.

I have been compelled to advert only in very brief terms to this important paper of Mr. Stone's, but I feel assured that any person who has given attention to it cannot fail to be impressed with the ability and skill with which the author has discussed the immense mass of observations constituting the groundwork of his researches.

The subject of atmospheric refraction is one which has occupied the attention of some of the most eminent astronomers and mathematicians of Europe from the time of Cassini and Newton down to the present day. The refractions now chiefly used by astronomers are those of Bessel, which were originally published in the *Fundamenta Astronomiæ*, and afterwards in a somewhat modified form in the *Tabulæ Regiomontanæ*. The theory, as is well known, is the same in both works, the only difference being that from the zenith to 85° the refractions in Bessel's later work, the *Regiomontanæ*, exceed in a slight degree those of the *Fundamenta*.

At Greenwich it has been the practice since 1857 to employ a still further modification of Bessel's refractions. From the zenith to 82° the mean refractions are those of the *Tabulæ Regiomontanæ* unaltered; from 82° to 85°, the refractions are those of the *Tabulæ Regiomontanæ* slightly diminished; while from 85° to the horizon, the mean refractions are those of the *Fundamenta* slightly diminished also. This arrangement was the result of an elaborate investigation of refraction by Mr. Main, founded partly on observations of Greenwich Circumpolar stars, and partly also on observations of South stars common to Greenwich and the Cape of Good Hope. No astronomer could be more deeply impressed than Mr. Main was with the want of symmetry which this arrangement exhibited, and, while recommending its adoption as indispensable in the existing state of matters, he regarded it merely as a provisional expedient to be supplanted at some future time by the results of a more advanced theory.

One of the objects proposed by Mr. Stone is to show that the

refractions of the *Tabulæ Regiomontanæ* from the zenith to 85° are somewhat too great.

The materials of his investigation consist partly of observations of Greenwich Circumpolar Stars, and partly of observations of stars common to Greenwich and Melbourne. The results, in so far as the latter place is concerned, refer exclusively to the years 1863, 1864, and 1865.

First, considering the case of the Greenwich Circumpolar stars, he arranges his materials in five groups, to each of which he assigns a weight according to the number of observations. He then forms an equal number of equations of condition involving a correction to the *Tabulæ Regiomontanæ*, and a correction to the co-latitude of Greenwich, and, solving them by the method of least squares, he obtains the values of the unknown quantities.

The result of this process shows that the refractions of the *Tabulæ Regiomontanæ*, down to 85° of zenith distance, are too great by a quantity of sensible magnitude, as is seen in the very considerable reduction of the residual quantities of the equations, arising from the substitution of the numerical values of the corrections.

Now Mr. Stone shows that the result of that correction is tantamount to diminishing the mean refractions of the *Fundamenta* in the proportion of 0·99797 to 1.

But at present the refractions below 85° employed at Greenwich are those of the *Fundamenta* diminished in the proportion of 0·99848 to 1.

Mr. Stone accordingly concludes that if the mean refractions of the *Fundamenta* diminished in the proportion of 0·99797 to 1 were adopted for Greenwich, there would hence result the advantage of being able to compute the refractions from the zenith to the horizon with one set of tables.

Mr. Stone then proceeds to investigate the correction for the refractions of Melbourne by means of observations of Circumpolar stars, and finally the corrections applicable to Greenwich and Melbourne respectively, by means of the observations of stars common to both Observatories. But it would occupy too much of your time if I were to enter into any further details. I may state briefly that the author obtains for the Greenwich refractions a correction the same in sign as that which he had previously derived from his discussion of the Greenwich circumpolar stars, although a correction of rather less magnitude.

With respect to the Melbourne refractions he obtains corrections agreeing in sign with the corrections to the Greenwich refractions, but differing from them in magnitude, and also differing sensibly for North and South stars. Indeed the discordances of the Melbourne observations are so considerable that Mr. Stone is inclined to look for their origin in some physical cause, and he suggests whether they may not be attributable to the fact of the strata of the atmosphere towards the south, at Melbourne, being more loaded with moisture than the strata towards the north.

I may add that the result of Mr. Stone's refraction investigation has been adopted for use at the Royal Observatory, Greenwich, with Mr. Airy's approbation.

I have thus, Gentlemen, endeavoured to give a faint outline of this paper of Mr. Stone's. Perhaps the time may not yet have arrived for a thorough re-casting of the great subject to which it refers. But of this I feel assured, that you will all agree with me in recognising the ability and skill which the author has displayed in his researches. The residual discordances which he has so clearly pointed out may not, under existing circumstances, admit of an explanation which is satisfactory in all respects, but the tracing out such anomalies cannot fail to lead to further advances in science.

We owe to Mr. Stone two papers on the Constant of Lunar Nutation which have been recently communicated to the Society.

In the first of these he has investigated the value of the nutation constant deducible from the observations in north polar distance of *Polaris*, *Cephei* 51, *δ Ursæ Minoris*, made with the Greenwich Transit Circle between the years 1851 and 1865 inclusive.

In his second paper he has investigated the value of the same element derivable from the observations in right ascension of *Polaris*, made also with the Greenwich Transit Circle between the years 1851 and 1867.

In the first of these investigations he had a difficulty to contend with arising from the circumstance of different division errors and different values of astronomical flexure having been employed successively in the reduction of the observations throughout the period of fifteen years over which they extended. Furthermore there were used in the process of reduction nadir points determined by a combination of reflecting eye-piece results with those obtained by observations of stars by reflexion.

The first step of the author, therefore, was to investigate the numerical corrections applicable to the registered north polar distances of the three stars, in order that the results might be regarded as equivalent to having been reduced according to one uniform system. Again a still further correction was required to be applied in consequence of the nutation constant used in the reductions during the earlier part of the period having been different from that used in the latter part.

Discussing, then, the observations of each star in succession the author arranges them in yearly groups, and in this manner forms a system of fifteen equations of condition involving three unknown quantities, a correction to the assumed nutation constant, a correction to the assumed north polar distance of the star, and a quantity representing the star's proper motion.

Finally, solving these equations, he obtains a value of the nutation constant affected by a quantity representing the proper motion of the star, which he leaves in the meantime undetermined.

In his second paper, which is devoted to a discussion of the mean right ascensions of *Polaris*, Mr. Stone similarly collects the observations into yearly groups, and by means of the resulting materials forms a system of seventeen equations of condition involving as unknown quantities the correction to the nutation constant, the correction to the assumed mean right-ascension of the star, and a correction to the proper motion of the star.

Solving, then, the equations by the method of least squares, he obtains the value of the nutation constant, affected with an undetermined correction to the proper motion of the star. Finally, adopting the proper motions of *Polaris* in R.A. and N.P.D., and of *Cephei* 51, and δ *Ursæ Minoris* in N.P.D., as given by Gould in his *Standard Places of Fundamental Stars*, he determines the definitive value of the constant of nutation to be $9''{\cdot}134$.

This result is slightly smaller than the result of any other value of the same constant which has been derived from the researches of astronomers in the present century, with the exception of the value obtained by Lindenau.

It is to be borne in mind, however, that the observations which form the groundwork of Mr. Stone's researches were all made with the same instrument, and that, too, one of the most powerful in existence for astronomical purposes, that they were carefully cleared of every known source of instrumental error, that they were all reduced according to one uniform system, and were finally subjected to a most thorough discussion.

Under these circumstances I feel bound to consider that the result obtained by Mr. Stone for the value of the nutation constant must be regarded as definitive, in so far as it is derivable from the Greenwich Observations of the present time.

In the former of his two papers on this subject Mr. Stone remarks that the corrections to the co-latitude, as determined from the yearly means of the observations of *Polaris* above and below pole, were affected by irregularities which seemed to indicate the existence of a systematic error proceeding from some undetermined cause. Assuming that these outstanding irregularities depended on the position of the Moon's node, he investigated the effects hence arising, and, upon comparing the results with those assigned by observation, he arrives at the conclusion that there may be really some grounds for supposing that the Greenwich co-latitude is affected by a periodical change. Admitting the existence of such a period he suggests whether it may not be attributable to a yielding of the Earth's crust under the Moon's action, or rather perhaps to a systematic deformation of the Earth's atmosphere arising from the same cause.

I have deemed it proper to bring under your notice this supplement of Mr. Stone's paper because from the admirable skill with which the author has eliminated every known source of error from the co-latitude corrections the outstanding anomalies acquire a value which would not otherwise attach to them.

Anomalous irregularities affecting the co-latitude of the

Greenwich Observatory, it is well known are not new. The true cause of these irregularities will no doubt be one day thoroughly ascertained. In the meantime any investigation such as that which we owe to Mr. Stone is invaluable as constituting a clue which may lead to their discovery.

It remains for me to glance briefly at one or two of the other papers which Mr. Stone has communicated to the Society.

It is well known that the stars when seen through a good telescope exhibit disks of sensible magnitude, surrounded to some distance beyond by an alternation of dark and bright rings.

The explanation of these rings upon the principles of the undulating theory of light is due to the Astronomer Royal (*Trans. Phil. Soc. Camb.* vol v.).

It may be shown by theory that the radius of any one of those rings varies inversely as the aperture.

In the *Monthly Notices* for December 1865 there is published a short paper by Mr. Stone upon this subject.

In this communication he has given measures from theory of the relative illumination of the disk at twenty different distances from the centres. The results show that there is a rapid degradation of the light of the disk towards the first black ring.

The author accordingly draws the conclusion that it by no means follows from the theory that the angular diameter of the *visible* disk varies inversely as the radius of the aperture.

Mr. Stone has contributed several papers to the *Monthly Notices*, having for their object the elucidation of certain interesting points in Physical Astronomy. I shall here confine myself to a brief allusion to one of these, a paper on the determination of the Moon's mass, which is published in the *Monthly Notices* for Jan. 1868.

In this communication to the Society Mr. Stone has investigated independently the expressions for lunisolar precession and nutation derivable from theory, and assigning to the constants of those elements their most accredited values, he hence determines the Moon's mass to be $\frac{1}{81\cdot36}$.

In a short paper which has been published in the *Monthly Notices*, Mr. Stone has considered the very important question of the circumstances which determine the rejection of discordant observations.

Distinguishing the ordinary class of graduated errors from errors due to accidental causes, he then proceeds to investigate a formula for the rejection of discordant observations, founded upon the assumption of a definite ratio of the mass of the observations being affected with errors due to accidental causes.

No doubt the assumption is to a certain extent arbitrary, but the principle is simple, and leads to a criterion for the rejection of discordant observations the arbitrariness of which is confined within comparatively narrow limits.

I have thus, Gentlemen, endeavoured to submit to you a state-

ment of the facts which have influenced the Council in their deliberations with reference to the award of the Medal for the present year.

The President then presented the Medal to Mr. Stone, addressing him in the following terms:—

Mr. Stone, in presenting you with this Gold Medal I know that I am not only placing in your hand the most honourable testimony of your great zeal and talent which it is in the power of this Society to bestow, but I feel convinced that every astronomer will recognise the award as a just tribute offered by the Society to the eminent ability, the energy, and assiduity which have distinguished your labours for the advancement of Astronomy.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

Admiral R. H. MANNERS.

Vice-Presidents:

J. C. ADAMS, M.A. F.R.S., Lowndean Professor of Astronomy, Cambridge.
A. CAYLEY, M.A. F.R.S., Sadlerian Professor of Geometry. Cambridge.
WARREN DE LA RUE, Ph. D. F.R.S.
Rev. CHARLES PRITCHARD, M.A. F.R.S.

Treasurer:

SAMUEL CHARLES WHITBREAD, Esq., F.R.S.

Secretaries:

WILLIAM HUGGINS, Esq. F.R.S.
EDWARD J. STONE, Esq. M.A. F.R.S.

Foreign Secretary:

Lieut.-Col. ALEXANDER STRANGE, F.R.S

Council:

G. B. AIRY, M.A. F.R.S., Astronomer Royal.
Rev. Professor CHALLIS, M.A. F.R.S.
JAMES HENRY DALLMEYER, Esq.
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J. NORMAN LOCKYER, Esq.
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Captain WILLIAM NOBLE.
BALFOUR STEWART, Esq. M.A. LL.D. F.R.S.
ISAAC TODHUNTER, Esq. M.A. F.R.S.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *March* 12, 1869. No. 5.

Admiral MANNERS, President, in the Chair.

Col. Addison, 2nd Regiment of Foot;
Chas. Barton, Esq., H.M.S. *Conway*, Rock Ferry, Liverpool;
Rev. R. Prichard Davies, Hatfield, Herts;
Lieut. George Strahan, Uxbridge Villas, Slough;

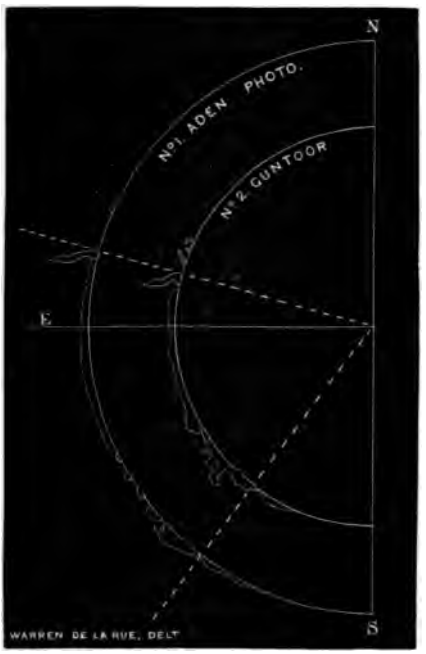
were balloted for and duly elected Fellows of the Society.

On the Aden and Guntoor Photographs of the Eclipse of the Sun, August 18, 1868. By Warren De La Rue, Esq.

In a communication published in the *Monthly Notices*, vol. xxix., No. 3, I called attention to differences in the representations of the prominence called the "Great Horn," as depicted in a woodcut of the Aden Photographs in the *Engineer* of November 6 and 13, 1868, and in the fac-similes of the drawings made by the several observers along the line of totality, when compared with the Guntoor photographs. I therein stated that these differences in the several aspects of the prominence in question might be accounted for on the supposition that it was undergoing an axial rotation during the period that it was being observed at the several stations.

I pointed out, however, that this hypothesis depended mainly upon the faithfulness of the copy of the Aden photograph, as given in the *Engineer*, and that it would not be possible to speak with certainty on this point until prints of the Aden and Guntoor

photographs had been compared. I had endeavoured to procure copies of the Aden photographs before I sent in my paper, but was not successful; since its publication, however, I have obtained a copy of No. 1 Aden photograph, through the kindness of Dr. Foerster, Director of the Berlin Observatory.



I have now the honour to lay before the Society a tracing of this photograph, combined with one of No. 2 Guntoor photograph, in such a manner that they may be directly compared.

It will be seen that there is a great difference between the woodcut of the Aden photograph in the *Engineer* and the tracing herein given; and that so far from the Great Horn being so curved that its point is directed northward, the point (as is the case in the Guntoor photographs) is directed southward; moreover, there is not much difference in the aspect of this prominence in these pictures taken 40 minutes apart. The inclination, N. towards E., however, of the point to a radius drawn tangentially to the northern boundary of the base is 6° less in the Aden photograph (24° 20′) than it is in the Guntoor photograph, 30° 20′. A comparison of the two pictures makes it evident, therefore, that some change in the direction of the Great Horn had occurred in the lapse of 40 minutes, but it is not possible to say with certainty whether or not it was due to an axial rotation of this prominence.

On the Transit of Mercury in 1868, *and on* γ^1, γ^2, *and* γ^3 *Andromedæ in Daylight.* By Thomas Barneby, Esq.

Having read accounts of the different appearances presented to different observers of the transit of *Mercury* across the disk of the Sun on the 5th November, 1868, I think an account of another observation of it, with a large refractor, may be interesting and perhaps useful.

In my locality the sky was cloudless and the air steady, and as soon as I could reach the Sun with my Equatoreal, at about $7^h\ 45^m$ G.M.T., I saw a well-defined very black but dull circular image of *Mercury* projected on the Sun. I watched it, with a friend, from that time until the time of the planet's disappearance at about $9^h\ 2^m\ 41^s$ G.M.T.

I saw no bright spot on the planet, nor any aureola around it, but I several times noticed on it a streak of light from south to north towards the west, in the shape of twisted lightning, which I attributed to ocular delusion.

I perceived no filament thrown out from the planet on approaching last internal contact, nor was there any distortion interfering with circularity, apparent to me of either celestial body during the progress of the transit, although the edge of each seemed slightly serrated.

I may mention, as a test of my telescope and of my sight, that on the 5th of February, 1869, at about $5^h\ 45^m$ G.M.T., with the same instrument, I saw γ^2 and γ^3 *Andromedæ* with a well-defined line of division between them, whilst there was daylight by which I read the R.A. and D. of their primary, and set the circles of my instrument.

γ^1 had a well-defined clean disk of a deep orange colour: γ^2 was a light yellow, and γ^3 distinctly blue. I observed to my companion, "The smaller star separated from the other is blue," without recollecting that it had been seen so before.

My view of the Sun was with a solar diagonal eye-piece, and a power of 113 applied to it, and that of γ *Andromedæ* with a direct eye-piece, and a power of 720, having the whole 9-inches aperture of my object-glass exposed on each occasion.

South Villa Observatory, near Worcester,
8 February, 1869.

Observations on the Transit of Mercury, 5th November, 1868, *Hobart Town.* By Thomas Abbott, Esq.

I transmit a diagram with the following observations, made with an equatoreally-mounted achromatic of 4-inches aperture and 5-feet focal length. Other observations were made at the same time and near the same place by an assistant, with a small Gregorian reflector. The day was more than usually fine, and the whole passage of the planet over the Sun well seen. According to the value imparted to such a transit for correcting or verifying the longitude, time being deduced from the *Nautical Almanac*, and when compared with the adopted longitude of the place, this transit, as near as observation could be made, was remarkably close.

The Royal Astronomical Society are in possession of all the means employed by Commander Kay for determining the geo-

graphical position of the Magnetic Observatory here, the whole series of observations having been reduced for them by Captain Shadwell, which gave for the longitude $9^h\ 49^m\ 29^s{\cdot}6$. Since the time this result was arrived at, Captain Denham, when stationed at Garden Island,* sent into these waters an officer and crew for the purpose of testing the longitude of Hobart Town. The result of all observations made at this time gave for the longitude $9^h\ 49^m\ 28^s{\cdot}8$, being a small difference of $0^s{\cdot}8$ (eight-tenths of a second). There remains, however, much doubt as to to the correct geographical position of any of the Australian colonies being well known. Captain Kay thinks with the Astronomer Royal that for observing the forthcoming transit of *Venus* they are not suited until better known. Mr. Ellery thinks differently, he considers the position of the Melbourne Observatory to be as well known as that of the Cape of Good Hope.

There is very great uncertainty of any expedition being able to reach either Wilks' Land or Sabrina Land in the month of December. If, therefore, some well-defined spot could be fixed upon, and suitable means employed to establish it as a starting-point for determining the correct geographical position of any other spot with equal accuracy, there would be found no difficulty in making correct, for the reception of any expedition that might be sent for the purpose, any position of the Australian Colonies, either by telegraphic communication, transmission of chronometers, or such other means as might be thought most suitable.

The Diagram accompanying the foregoing paper shows—

	h	m	s	
External contact at Ingress	3	14	9	Hobart Town M.T.
,, ,, Egress	6	52	27	,,

and least distance of line of transit from centre $= 12'\ 15''$, the Sun's semidiameter being $16'\ 11''$.

Private Observatory,
Hobart Town, Tasmania,
28 *December*, 1868.

Observations of the Transit of Mercury over the Sun's Disk on Nov. 4th, 1868, at the Royal Observatory, Cape of Good Hope, with Equations deduced therefrom. By W. Mann, First Assistant at the Observatory.

With the 8½-foot Equatoreal, the following observations at ingress and egress were made by Sir T. Maclear and Mr. G. Maclear.

* The established place of observation for all the officers of H.M. ships visiting Sydney.

Ingress.

	Chron. Times. h m s	Cape M.T. h m s	Obs.	Remarks.
Mercury apparently bisected by the Sun's limb	18 48 16·7	18 40 3·5	T.M.	Good.
Interior contact of limbs ..	18 49 48·0	18 41 34·80	G.M.	Good.

Egress.

Interior contact of limbs ..	22 23 0·0	22 14 46·68	T.M.	
Mercury apparently bisected by the Sun's limb	22 24 16·0	22 16 2·68	T.M.	
Exterior contact of limbs ..	22 25 20·5	22 17 7·18	T.M.	

The sky was partially and thinly clouded at egress, through which the objects were seen with sufficient clearness, but the limbs were badly defined and tremulous.

The powers used were 200 at the ingress, and 440 at the egress.

The following observations were made with a divided object-glass micrometer, attached to the Dollond telescope of 46-in. focal length, and $3\frac{1}{2}$-in. aperture, employing a power of 80 throughout, and they consist of measures of the distances of both limbs of *Mercury* from the nearest and furthest points on the Sun's limb.

The four different classes of observation are distinguished by the letters A, B, C, D, thus :—

A denotes an internal contact of the limb of *Mercury* with the nearest point on the Sun's S. limb.

B denotes an external contact of the limb of *Mercury* with the nearest point on the Sun's S. limb.

C denotes an internal contact of the limb of *Mercury* with the furthest point on the Sun's N. limb.

D denotes an external contact of the limb of *Mercury* with the furthest point on the Sun's N. limb.

The plan of operations it was proposed to follow during the progress of the transit was only partially and imperfectly carried out, owing to the unfavourable state of the sky.

From the commencement to set (6), the sky was clear, and the images of the Sun and planet, although faint, were sharp and steady; but from set (7) to the close, the Sun was repeatedly clouded, and the observations for the most part were obtained with great difficulty, and their accuracy undoubtedly much impaired by the extreme faintness and indistinctness of the images.

With three exceptions, five measures of each contact of the limbs were obtained; and the mean of the measures is considered sensibly to correspond with the mean of the times.

The following table contains all the measures that were taken, with the chronometer times, and the means of each group.

No. of Set.	Class of Obs.	Chron. Times. h m s	Scale Reading.	Remarks.
1	A	18 51 10	0·081	
		52 12	·090	
		53 34	·104	
		54 45	·116	
		55 28	·123	
		18 53 25·8	0·1028	
2	B	18 57 45	0·162	
		58 21	·168	
		58 56	·174	
		59 40	·178	
		60 16	·184	
		18 58 59·6	0·1732	
3	C	19 4 22	4·433	
		5 30	·430	
		6 2	·422	
		7 5	·414	
		7 35	·409	
		19 6 2·8	4·4126	
4	D	19 8 18	4·424	
		8 57	·420	
		9 42	·414	
		10 13	·411	
		11 16	·402	
		19 9 41·2	4·4142	
5	A	19 15 28	0·273	
		16 7	·280	
		16 45	·282	
		17 17	·286	
		18 10	·293	
		19 16 45·4	0·2824	
6	B	19 19 21	0·320	
		19 58	·324	
		20 43	·333	
		21 19	·338	
		22 2	·344	
		19 20 40·6	0·3318	

No. of Set.	Class of Obs.	Chron. Times. h m s	Scale Reading.	Remarks.
7	C	19 25 8	4·278	Clouded; both objects very faint.
		25 52	·275	
		26 28	·272	
		27 10	·269	
		28 15	·260	
		19 26 34·6	4·2708	
8	D	19 29 18	4·274	Clouded; both objects very faint.
		30 36	·272	
		32 40	·258	
		33 30	·253	
		34 23	·247	
		19 32 5·4	4·2608	
9	A	19 37 24	0·412	Clouds.
		38 30	·419	
		39 13	·421	
		39 58	·425	
		43 35	·442	
		19 39 44·0	0·4238	
10	B	19 45 12	0·474	The Sun frequently obscured by cloud.
		46 35	·482	
		48 32	·490	
		51 8	·498	
		53 6	·510	
		19 48 54·6	0·4908	
11	D	20 5 3	4·123	In these measures the contact at the S. limb of *Mercury* instead of the N. limb was inadvertently taken.
		5 26	·125	
		6 7	·122	
		20 5 32·0	4·1233	
12	C	20 16 22	4·083	From 20^h to $20^h\ 30^m$ a few glimpses only of the Sun were obtained.
		16 58	·076	
		20 16 40	4·0795	
13	D	20 30 54	4·090	After taking this set the Sun became wholly obscured for about 45^m.
		31 38	·091	
		32 18	·097	
		33 0	·097	
		33 32	·098	
		20 32 16·4	4·0946	

No. of Set.	Class of Obs.	Chron. Times. h m s	Scale Reading.	Remarks.
14	A	21 36 7	0·310	
		36 44	·306	
		37 25	·300	
		38 12	·298	
		38 40	·292	
		21 37 25·6	0·3012	
15	B	21 39 25	0·311	
		39 55	·307	
		40 22	·308	
		41 8	·300	
		41 55	·295	
		21 40 33·0	0·3042	
16	C	21 46 20	4·388	The Sun clouded; the observations very uncertain.
		48 16	·408	
		49 0	·406	
		51 42	·430	
		52 23	·432	
		21 49 33·2	4·4128	
17	D	21 53 5	4·457	Clouded; the observations very uncertain.
		53 40	·459	
		54 17	·463	
		55 5	·477	
		55 45	·478	
		21 54 22·4	4·4668	
18	A	22 7 44	0·064	
		8 4	·066	
		8 25	·062	
		9 0	·055	
		9 28	·051	
		22 8 32·2	0·0596	
19	B	22 10 0	0·064	Very faint; further observation impracticable.
		10 32	·060	
		22 10 16·0	0·0620	

The above measures were made by myself, the scale being read by Mr. Sinfield.

The chronometer errors, obtained from comparisons with the transit-clock before and after the observations were, at $18^h\ 16^m\ 5^s$,

chronometer fast $23^s{\cdot}75$, and at $22^h\ 28^m\ 0^s$ fast, $24^s{\cdot}09$ on Cape Mean Solar Time.

The scale value in arc was deduced from a set of measures of the Sun's horizontal diameter at $22^h\ 45^m$; twelve measures gave 4·6540 in., or corrected for index error, 4·6567 for the apparent diameter; and the tabular value being $32'\ 21''{\cdot}56$, we have

$$1 \text{ inch on the Scale} = \frac{1941''{\cdot}56}{4{\cdot}6567} = 416''{\cdot}94.$$

The index error of the scale was derived from contacts of the moveable image of *Mercury* at alternate sides of the fixed image. Six positive and six negative readings gave 0·0027-in. for the correction to be *added* to a scale reading.

The following table contains an abstract of the means of the foregoing groups of observations, the times being corrected for chronometer error, and the scale readings corrected for index error, and converted into arc.

Set.	Class of Measure.	Cape Mean Times. h m s	Measure. ′	No. of Measures.
1	A	18 53 2·0	43·99	5
2	B	58 35·8	73·34	5
3	C	19 5 39·0	1844·66	5
4	D	9 17·4	1841·58	5
5	A	16 21·6	118·87	5
6	B	20 16 8	139·47	5
7	C	26 10·8	1781·79	5
8	D	31 41·5	1777·62	5
9	A	39 20·1	177·82	5
10	B	48 30·7	205·76	5
11	D	20 5 8·1	1720·29	3
12	C	16 16·1	1702·03	2
13	D	31 52·5	1708·32	5
14	A	21 37 1·6	126·71	5
15	B	40 9·0	127·96	5
16	C	49 8·2	1841·00	5
17	D	53 58·4	1863·50	5
18	A	22 8 8·1	25·98	5
19	B	9 51·9	26·98	2

In addition to the foregoing, the times of external and internal contact at ingress were noted as follow:—

	h m s		h m s	
External contact of limbs	18 40 6	by chron. or	18 39 42·2	Cape M.T.
Internal contact ,,	18 41 58	,,	18 41 34·2	,,

The time of external contact is of little value, and the instant

of internal contact is not very accurate, owing to an indistinctness of limb caused by a slight overlap of the images in a direction at right angles to the line of section of the lenses.

In consequence of this error of adjustment—for the correction of which no means exist in this instrument—measures of the diameter of *Mercury* were not attempted with the divided object-glass.

Reduction of the Observations.

The first step was the calculation of the apparent distances of the centres of the Sun and *Mercury* for the times of ingress and egress observed with the 8½-foot Equatoreal, taking the contacts of the limbs only; also for the times given in the foregoing abstract of measures, and with elements derived from the *Nautical Almanac*.

With Greenwich Mean Solar times corresponding to the above Cape Mean Solar times, the Right Ascensions and South Polar Distances of the Sun and *Mercury* were interpolated with second and third differences.

The parallaxes in Right Ascension and Polar Distance were calculated with the aid of tables adapted to the latitude of the place, and of a similar form to those described in various Introductions to the Greenwich Observations, containing quantities D and θ, calculated for every minute of hour-angle, and determined by the formulæ

$$\sin D = \text{sin geocentric colat.} \times \text{sin hour-angle},$$
$$\tan \theta = \text{tan geocentric colat.} \times \cos \text{hour-angle},$$

with which we obtain

$$\text{the Parallax in R.A. (in arc)} = \frac{\text{hor. eq. parallax} \times r \times \sin D}{\sin \text{S.P.D.}}$$

$$\text{the Parallax in P.D.} = \text{hor. eq. parallax} \times r \,.\, \cos D \,.\, \sin (\text{S.P.D.} - \theta)$$

r being the geocentric radius for the Cape, in terms of the equatoreal radius.

The tabular and the apparent Right Ascensions and South Polar Distances of the Sun and *Mercury* are exhibited in the subjoined table. The numerals I, II, and III, in the first column serve to distinguish the quantities depending on the time of internal contact at ingress and the times of internal and external contact at egress; the other numerals refer to the observations made with the divided object-glass.

In the calculation of the parallaxes in R.A. and S.P.D. the tabular equatoreal horizontal parallax of *Mercury* was taken $= 12''{\cdot}70$, and that of the Sun $= 8''{\cdot}66$.

No. of Set.	Sun's Tabular R.A.	Par. in R.A.	Sun's Apparent R.A.	Sun's Tabular S.P.D.	Par. in S.P.D.	Sun's Apparent S.P.D.
	° ′ ″	″	° ′ ″	° ′ ″	″	° ′ ″
I	220 39 20·07	7·24	220 39 27·31	74 12 57·85	4·14	74 13 1·99
1	39 48·51	7·14	39 55·65	12 49·18	4·04	12 53·22
2	40 2·36	7·08	40 9·44	12 44·96	4·00	12 48·96
3	40 19·88	7·00	40 26·88	12 39·62	3·94	12 43·56
4	40 28·94	6·96	40 35·90	12 36·86	3·91	12 40·77
5	40 46·52	6·87	40 53·39	12 31·51	3·86	12 35·37
6	40 56·25	6·82	41 3·07	12 28·54	3·83	12 32·37
7	41 10·92	6·74	41 17·66	12 24·07	3·78	12 27·85
8	41 24·62	6·66	41 31·28	12 19·90	3·74	12 23·64
9	41 43·64	6·54	41 50·18	12 14·11	3·68	12 17·79
10	42 6·45	6·39	42 12·84	12 7·16	3·61	12 10·77
11	42 47·78	6·10	42 53·88	11 54·58	3·49	11 58·07
12	43 15·48	5·88	43 21·36	11 46·15	3·42	11 49·57
13	43 54·27	5·55	43 59·82	11 34·35	3·32	11 37·67
14	46 36·12	3·93	46 40·05	10 45·05	2·96	10 48·01
15	46 44·06	3·84	46 47·90	10 42·69	2·94	10 45·63
16	47 6·41	3·58	47 9·99	10 35·89	2·90	10 38·79
17	47 18·44	3·44	47 21·88	10 32·24	2·88	10 35·12
18	47 53·66	3·03	47 56·69	10 21·53	2·83	10 24·36
19	47 57·98	2·98	48 0·96	10 20·22	2·83	10 23·05
II	48 10·20	2·83	48 13·03	10 16·51	2·81	10 19·32
III	220 48 16·02	2·76	220 48 18·78	74 10 14·73	2·80	74 10 17·53

No. of Set.	Mercury's Tabular R.A.	Par. in R.A.	Mercury's Apparent R.A.	Mercury's Tabular S.P.D.	Par. in S.P.D.	Mercury's Apparent S.P.D.
	° ′ ″	″	° ′ ″	° ′ ″	″	° ′ ″
I	220 43 27·54	10·63	220 43 38·17	73 57 23·65	6·05	73 57 29·70
1	42 53·12	10·48	43 3·60	57 44·49	5·91	57 50·40
2	42 36·39	10·40	42 46·79	57 54·62	5·84	58 0·46
3	42 15·20	10·28	42 25·48	58 7·45	5·76	58 13·21
4	42 4·25	10·22	42 14·47	58 14·09	5·71	58 19·80
5	41 43·01	10·09	41 53·10	58 26·95	5·63	58 32·58
6	41 31·22	10·02	41 41·24	58 34·09	5·59	58 39·68
7	41 13·49	9·90	41 23·39	58 44·82	5·52	58 50·34
8	40 56·93	9·78	41 6·71	58 54·85	5·45	59 0·30
9	40 33·95	9·61	40 43·56	59 8·77	5·37	59 14·14
10	40 6·39	9·38	40 15·77	59 25·47	5·27	59 30·74
11	39 16·47	8·94	39 25·41	59 55·72	5·09	74 0 0·81
12	38 43·02	8·62	38 51·64	74 0 15·99	4·98	0 20·97
13	37 56·15	8·14	38 4·29	0 44·40	4·83	0 49·23
14	34 40·58	5·73	34 46·31	2 43·01	4·31	2 47·32
15	34 31·22	5·60	34 36·82	2 48·69	4·29	2 52·98
16	34 4·25	5·22	34 9·47	3 5·06	4·23	3 9·29
17	33 49·76	5·02	33 54·78	3 13·86	4·20	3 18·06

No. of Set.	Mercury's Tabular R.A.	Par. in R.A.	Mercury's Apparent R.A.	Mercury's Tabular S.P.D.	Par. in S.P.D.	Mercury's Apparent S.P.D.
	° ′ ″	″	° ′ ″	° ′ ″	″	° ′ ″
18	220 33 7·28	4·40	220 33 11·68	74 3 39·64	4·13	74 3 43·77
19	33 2·09	4·32	33 6·41	3 42·79	4·12	3 46·91
II	32 47·35	4·11	32 51·46	3 51·74	4·09	3 55·83
III	220 32 40·33	4·00	220 32 44·33	3 56·00	4·08	74 4 0·08

If now we put

a for the excess of the apparent R.A. of the Sun's centre above that of *Mercury's* centre;

p for the excess of the apparent S.P.D. of the Sun's centre above that of *Mercury's* centre;

Δ and δ for the S.P.D.'s of the Sun and *Mercury*;

then, the apparent distance of the centres $(c) = p \sec \zeta$,

$$\text{where } \tan \zeta = \frac{a}{p} \sqrt{\sin \Delta \sin \delta}.$$

Also, let σ and μ be the semidiameters of the Sun and *Mercury*, and m' the tabular value of the measured distance of the limbs; then for the several classes of observations with the divided object-glass micrometer, we shall have

For Class A, $m' = \sigma - \mu - c$, Class C, $m' = \sigma - \mu + c$,

B, $m' = \sigma + \mu - c$, D, $m' = \sigma + \mu + c$,

At the instant of ingress or egress, $m' = 0$

and we have

For the internal contact, $c = \sigma - \mu$; for the external contact, $c = \sigma + \mu$.

Substituting the tabular values of σ and μ, viz. $\sigma = 970''{\cdot}78$ and $\mu = 4''{\cdot}94$, we have

For A, $m' = 965''{\cdot}84 - c$ for C, $m' = 965''{\cdot}84 + c$

B, $m' = 975''{\cdot}72 - c$ D, $m' = 975''{\cdot}72 + c$

The values of a, p, c, and m', are given below:—

No. of Set.	Class of Observation.	a.	p.	c.	m'.
		′ ″	′ ″	″	″
I	A	− 4 10·86	15 32·29	963·00	0·00
1	A	3 7·95	15 2·82	920·74	45·10
2	B	2 37·35	14 48·50	901·29	74·43
3	C	1 58·60	14 30·35	877·79	1843·63
4	D	1 38·57	14 20·97	866·17	1841·89
5	A	0 59·71	14 2·79	844·74	121·10
6	B	0 38·17	13 52·69	833·50	142·22
7	C	− 0 5·73	13 37·51	817·53	1783·37

No. of Set.	Class of Observation.	*a.*	*p.*	*c.*	*m.*
		′ ″	′ ″	″	″
8	D	+ 0 24·57	13 23·34	803·69	1779·41
9	A	1 6 62	13 3·65	786·26	179·58
10	B	1 57·07	12 40·03	768·32	207·40
11	D	3 28·47	11 57·26	744·75	1720·47
12	C	4 29·72	11 28·60	735·84	1701·68
13	D	5 55·53	10 48·44	733·07	1708·79
14	A	11 53·74	8 0·69	838·04	127·80
15	B	12 11·08	7 52·65	847·25	128·47
16	C	13 0·52	7 29·50	875·00	1840·84
17	D	13 27·10	7 17·06	890·86	1866·58
18	A	14 45·01	6 40·59	940·77	25·07
19	B	14 54·55	6 36·14	947·22	28·50
II	A	15 21·57	6 23·49	965·79	0·00
III	B	+15 34·45	6 17·45	974·82	0·00

Correction of the Measured Distances of the Limbs for the Effects of Refraction.

Calling the vertical refraction $= \alpha \tan z$, where z is the apparent zenith distance; also putting γ for the inclination of the measured distance m, to the vertical circle passing through its middle point; then the effect of the refraction on the measured distance will be given by the formula

$$\alpha m \sec^2 \psi \sin 1'', \text{ where } \tan \psi = \cos \gamma \tan z.$$

It will be sufficiently accurate for all the cases considered in the present discussion to calculate z and γ for the centre of the Sun at the instant of observation.

With the Sun's hour-angle and S.P.D, and with the aid of tables calculated for the latitude of the Cape, having arguments to every minute of hour-angle, the values of z_0 the true zenith distance, and of S. his angle of situation were readily obtained. The true zenith distances were easily converted into apparent Z.D.'s with the aid of a table of refractions; and the values of γ were found with all requisite accuracy from the expression $\gamma = S - \zeta$, where ζ, which is already calculated, is the inclination of the distance of the centres c to the circle of declination passing through its middle point.

The principal numerical quantities involved in the calculation of the corrections for refraction, together with the measures corrected and compared with the tabular values are given below.

The following are the readings of the barometer and thermometer used in calculating the values of log α from the table of refraction:—

Set 1 to 10	Set 11 to 13	Set 14 to 19
Bar. 30^{in}·19	Bar. 30^{in}·20	Bar. 30^{in}·22
Ther. { Att. 58°·0 / Out 54°·0	Ther. { Att. 59° / Out 56°	Ther. { Att. 60° / Out 58°·5

Set.	Sun's Hour Angle.	Apparent Z.D. of Sun's Centre (z).	Angle of Situation (s).	ξ.	γ.
	h m s	° ′	° ′	° ′	° ′
1	4 50 42	67 2	59 19	−11 19	70 38
2	4 45 8	65 53	59 24	9 40	69 4
3	4 38 5	64 25	59 29	7 28	66 57
4	4 34 26	63 40	59 31	6 17	65 48
5	4 27 32	62 14	59 33	3 54	63 27
6	4 23 27	61 23	59 34	2 31	62 5
7	4 17 33	60 10	59 34	− 0 23	59 57
8	4 12 2	59 2	59 33	+ 1 41	57 52
9	4 4 24	57 27	59 29	4 45	54 49
10	3 55 13	55 32	59 22	8 26	50 56
11	3 38 36	52 7	59 1	15 37	43 24
12	3 27 28	49 49	58 39	20 39	38 0
13	3 11 51	46 37	57 57	27 48	30 9
14	2 6 42	33 44	51 39	55 0	3 21
15	2 3 35	33 9	51 10	56 6	4 56
16	1 54 36	31 29	49 38	59 5	9 27
17	1 49 45	30 36	48 40	60 37	11 57
18	1 35 36	28 6	45 31	64 48	19 17
19	1 33 52	27 48	45 5	+65 17	20 12

Set.	Log a.	Corr. for Refr.	Measured Distance.	Meas. Dist. Corr. for Refr. (m).	Calculated Distance. (m').	$m - m'$.
		″	″	″	″	″
1	1·7619	0·02	43·99	44·01	45·10	−1·09
2	1·7622	0·03	73·34	73·37	74·43	−1·06
3	1·7624	0·86	1844·66	1845·52	1843·63	+1·89
4	1·7626	0·87	1841·58	1842·45	1841·89	+0·56
5	1·7628	0·06	118·87	118·93	121·10	−2·17
6	1·7630	0·07	139·47	139·54	142·22	−2·68
7	1·7631	0·88	1781·79	1782·67	1783·37	−0·70
8	1·7633	0·89	1777·62	1778·51	1779·41	−0·90
9	1·7634	0·09	177·82	177·91	179·58	−1·67
10	1·7636	0·11	205·76	205·87	207·40	−1·53
11	1·7623	0·90	1720·29	1721·19	1720·47	+0·72
12	1·7624	0·89	1702·03	1702·92	1701·68	+1·24
13	1·7626	0·88	1708·32	1709·20	1708·79	+0·41
14	1·7611	0·05	126·71	126·76	127·80	−1·04
15	1·7611	0·05	127·96	128·01	128·47	−0·46
16	1·7611	0·70	1841·00	1841·70	1840·84	+0·86
17	1·7611	0·70	1863·50	1864·20	1866·58	−2·38
18	1·7612	0·01	25·98	25·99	25·07	+0·92
19	1·7612	0·01	26·98	26·99	28·50	−1·51

Formation of Equations of Condition involving Corrections to the Elements of Computation.

Let

x be the correction to the tabular excess of the Sun's R.A. above *Mercury's* R.A. (in arc);

y be the correction to the tabular excess of the Sun's S.P.D. above *Mercury's* S.P.D.

$d\pi$ the correction to the excess of the parallax of *Mercury* above that of the Sun;

$d\rho$ and $d\mu$ the corrections to the semidiameters of the Sun and *Mercury*.

Also, let

τ seconds be the correction to the assumed longitude of the Observatory,

that is to say, let the true longitude be

$$-1^h\,13^m\,55^s + \tau \text{ sec.}$$

then, since the relative motions in R.A. and S.P.D. in 1^s of time are

$$+0''{\cdot}0915 \text{ and } -0''{\cdot}0429 \text{ respectively,}$$

we shall have the corrections to a and p, or da and dp, sensibly, as follows:—

$$da = x + {\cdot}0915\,\tau - \kappa\, d\pi$$
$$dp = y - {\cdot}0429\,\tau - \kappa'\, d\pi$$

κ and κ' being the coefficients of parallax in R.A. and S.P.D.

Now, since the corrections are all small, we have from the expression $C^2 = a^2 \sin \Delta \sin \delta + p^2$, considering C, a, and p only to vary,

$$dc = \frac{a}{c} \sin \Delta \sin \delta \,.\, da + \frac{p}{c}\, dp,$$

and the final equations for the observed times of ingress and egress will be—

For I. and II. $\rho - \mu - c = dc - d\rho + d\mu$; for III. $\rho + \mu - c = dc - d\rho - d\mu$.

Also,

$$\text{putting } m = m' + dm',$$

m being the measured and m' the computed distance of the limbs, the final equations for the observations made with the divided object-glass micrometer will be

For class A, $dm' = d\rho - d\mu - dc$ For class C, $dm' = d\rho - d\mu + dc$

B, $dm' = d\rho + d\mu - dc$ D, $dm' = d\rho - d\mu + dc$

But since a correction $d\sigma$ to the Sun's semidiameter will cause

a change in the arc value of the measured quantity m, $d\sigma$ in the four last equations must be multiplied by a factor of the form $(1-q)$, where $q = 2\frac{m}{m_0}$, m_0 being the scale measure of the Sun's diameter.

Introducing the numerical value of da, dp, and c, and reducing, we obtain finally—

No. of Equation.							
I.	$-2\cdot84 =$	$+1\cdot000\,de$	$-1\cdot000\,d\mu$	$+0\cdot241x$	$-\cdot968\,y$	$+\cdot064\,\tau$	$+\cdot260\,d\Pi$
1	$-1\cdot09 =$	$+0\cdot955$	$-1\cdot000$	$+0\cdot189$	$-\cdot981$	$+\cdot059$	$+\cdot301$
2	$-1\cdot06 =$	$+0\cdot925$	$+1\cdot000$	$+0\cdot162$	$-\cdot986$	$+\cdot057$	$+\cdot322$
3	$-1\cdot89 =$	$+0\cdot900$	$+1\cdot000$	$+0\cdot125$	$-\cdot992$	$+\cdot054$	$+\cdot349$
4	$-0\cdot56 =$	$+0\cdot897$	$-1\cdot000$	$+0\cdot105$	$-\cdot994$	$+\cdot052$	$+\cdot363$
5	$-2\cdot17 =$	$+0\cdot878$	$-1\cdot000$	$+0\cdot065$	$-\cdot998$	$+\cdot049$	$+\cdot391$
6	$-2\cdot68 =$	$+0\cdot856$	$+1\cdot000$	$+0\cdot042$	$-\cdot999$	$+\cdot047$	$+\cdot407$
7	$+0\cdot70 =$	$+0\cdot835$	$+1\cdot000$	$+0\cdot007$	$-1\cdot000$	$+\cdot044$	$+\cdot430$
8	$+0\cdot90 =$	$+0\cdot831$	$-1\cdot000$	$-0\cdot028$	$-1\cdot000$	$+\cdot040$	$+\cdot452$
9	$-1\cdot67 =$	$+0\cdot817$	$-1\cdot000$	$-0\cdot078$	$-\cdot997$	$+\cdot036$	$+\cdot482$
10	$-1\cdot53 =$	$+0\cdot788$	$+1\cdot000$	$-0\cdot141$	$-\cdot989$	$+\cdot030$	$+\cdot515$
11	$-0\cdot72 =$	$+0\cdot773$	$-1\cdot000$	$-0\cdot259$	$-\cdot963$	$+\cdot818$	$+\cdot569$
12	$-1\cdot24 =$	$+0\cdot754$	$+1\cdot000$	$-0\cdot339$	$-\cdot936$	$+\cdot009$	$+\cdot597$
13	$-0\cdot41 =$	$+0\cdot760$	$-1\cdot000$	$-0\cdot449$	$-\cdot885$	$-\cdot003$	$+\cdot625$
14	$-1\cdot04 =$	$+0\cdot870$	$-1\cdot000$	$-0\cdot788$	$-\cdot574$	$-\cdot048$	$+\cdot551$
15	$-0\cdot46 =$	$+0\cdot868$	$+1\cdot000$	$-0\cdot798$	$-\cdot558$	$-\cdot049$	$+\cdot541$
16	$-0\cdot86 =$	$+0\cdot896$	$+1\cdot000$	$-0\cdot825$	$-\cdot514$	$-\cdot054$	$+\cdot512$
17	$+2\cdot38 =$	$+0\cdot920$	$-1\cdot000$	$-0\cdot838$	$-\cdot491$	$-\cdot056$	$+\cdot495$
18	$+0\cdot92 =$	$+0\cdot973$	$-1\cdot000$	$-0\cdot870$	$-\cdot426$	$-\cdot061$	$+\cdot441$
19	$-1\cdot51 =$	$+0\cdot972$	$+1\cdot000$	$-0\cdot874$	$-\cdot418$	$-\cdot062$	$+\cdot434$
II.	$-0\cdot05 =$	$+1\cdot000$	$-1\cdot000$	$-0\cdot883$	$-\cdot397$	$-\cdot064$	$+\cdot415$
III.	$-0\cdot90 =$	$+1\cdot000$	$+1\cdot000$	$-0\cdot887$	$-\cdot387$	$-\cdot065$	$+\cdot405$

These equations alone are obviously unfitted for the determination of all the unknown quantities; and, moreover, considering the circumstances under which many of the observations were taken, we should not be justified in assigning an equal weight to them throughout. Values of the weights might be inferred from a consideration of the individual observations, but I have not prosecuted the inquiry further, under the belief that, when the objects were seen faintly through cloud, *constant* errors in the measured distances were as much to be feared as *accidental* errors of observation.

Approximate values of $d\mu$, x and y may be obtained as follows:—

If we take as one equation the mean of Nos. 11 and 13 (No. 11 having been observed by mistake at the S. limb of *Mercury*), and if we exclude I., the remaining twenty equations will give a

series depending upon equally-balanced observations at both limbs of the planet.

Changing the signs of the equations containing negative values of $d\mu$, and adding, we get

$$20\,d\mu = -8''{\cdot}485 + {\cdot}114\,d\sigma + {\cdot}148\,x - {\cdot}003\,y + {\cdot}003\,\tau - {\cdot}024\,d\pi,$$

or sensibly,

$$d\mu = -0''{\cdot}424.$$

Again, adding together equations I to 10, and equations II. to III., considering as before, the mean of 11 and 13 to form one equation, $d\mu$ is eliminated, and we have—

$$-11''{\cdot}05 = 8{\cdot}682\,d\sigma + 0{\cdot}448\,x - 9{\cdot}936\,y + 0{\cdot}468\,\tau + 4{\cdot}012\,d\pi,$$
$$-3''{\cdot}225 = 9{\cdot}020\,d\sigma - 7{\cdot}456\,x - 5{\cdot}625\,y - 0{\cdot}443\,\tau + 4{\cdot}998\,d\pi,$$

whence

$$x = -0''{\cdot}380 + 0{\cdot}532\,d\sigma - 0{\cdot}092\,\tau + 0{\cdot}352\,d\pi,$$
$$y = +1''{\cdot}095 + 0{\cdot}898\,d\sigma + 0{\cdot}043\,\tau + 0{\cdot}420\,d\pi.$$

Assuming the parallaxes to be increased by $\frac{1}{30}$th part, we have $d\pi = +0''{\cdot}13$; and taking $d\sigma = -1''{\cdot}1$, the correction given by the Greenwich Observations of the solar eclipse of 1860, we obtain finally, bearing in mind that y is the correction to the excess of S.P.D.—

Correction to excess of Sun's R.A. over *Mercury's* R A. $= -0''{\cdot}92 - {\cdot}092\,\tau$,
Correction to excess of Sun's N.P.D. over *Mercury's* N.P.D. $= -0''{\cdot}16 - {\cdot}043\,\tau$.

Sir T. Maclear, Director of the Observatory, writes:—

I annex the following, derived from my observations with the $8\frac{1}{2}$-foot Equatoreal, to Mr. Mann's elaborate discussion of his observations with the other instruments.

My wish to obtain a good determination of the diameter of *Mercury* was in a great measure frustrated by frequent interruptions from clouds and indifferent definitions after the first hour of the transit.

Under the powers 200 and 440 the planet presented the image of a black round disk, and (when the definition was good) surrounded by a very faint appearance of light. G. W. H. Maclear observed the first internal contact. I looked immediately after and noted three or four minute black dots in the narrow space between the limbs. I fancied something similar immediately before the last internal contact, but the limbs then were in violent apparent motion. I used a blue-glass eye-screen fixed to a diagonal eye-piece.

The micrometer of the 8½-foot Equatoreal has only one micrometer screw. There is a zero wire parallel to the micrometer wire, which can be placed at any part of the field. Therefore the telescope must be moved to supply the place of a second micrometer, by trial and error tapping with the finger.

The measures for diameter were in polar distance and by "cross" observation to eliminate the coincidence of the wires. Thus each recorded measure embraced twice the diameter.

One revolution of the micrometer-screw = 26″·069, and the sum of the thickness of the wires = 1″·737.

Each of the following results is the mean of five partial measures of the double diameter:—

Cape Mean Time. h m					
18 59	...	...	...	0′·5662	
19 12	...	...	...	0·5708	
19 22	...	...	...	0·5970	
21 47	...	...	...	0·5818	
21 59	...	...	...	0·5636	
	Mean	...	...	= 0·5759	= 15″·013
					7·507
	Half thickness of wires				·869
	Observed diameter				8·376 Probable error ±0·11.

This result is less than the tabular diameter by 1″·50.

On the Observations of the Transit of Venus in 1874.

By G. B. Airy, Esq., Astronomer Royal.

In the *Compte Rendu* of the French Academy for 1869, February 8, M. V. Puiseux has courteously adverted to my late paper on the Transits of *Venus*, and has suggested that the Transit of 1874 can be advantageously observed by the method of comparison of the intervals between ingress and egress, the ingress and egress being both observed at each of the places adopted fo comparison. And he has pointed out stations which give a differ ence of such intervals a very little larger than the intervals whic I have adopted, and intimates that the method suggested by hi is perhaps, therefore, superior to mine.

It will be seen in the *Monthly Notices* for 1857, May 8, p 213, second paragraph, and pages 214 and 215, that I had f considered the method which M. Puiseux has now suggested, that I had actually named the principal stations which M. Puis has now indicated. The matter having thus been fully pre to my mind, I can at once explain my reasons for preferring course which I have lately placed before the Society.

In M. Puiseux's plan the available time-determination will depend on the aggregate (not the mean) of four observations of ingress and egress. In my plan the available time-determination will depend on the aggregate of two observations of ingress or egress and two determinations of geographical longitude. And the practical question is, Which of these methods is subject to the smaller probable error?

Now I hope that, with reasonable care, the probable error of the geographical longitude will not be more than one-half of the probable error of ingress or egress. Moreover, the geographical position remains, and observations can be repeated in future years for the correction of its longitude; but nothing can repair error in the observation of ingress or egress. These points being considered, in combination with the circumstance that the effects of parallax obtained are sensibly equal, I cannot hesitate in maintaining the opinion that my method is the more accurate.

But I trust that the plan proposed by M. Puiseux will still be carried out. Every series of observations which can really be brought to bear upon this important determination will be valuable.

Royal Observatory, Greenwich,
1869, *February* 23.

Note on the Transit of Venus in 1874; *and an exact Determination of those Points on the Earth's Surface at which internal contacts are most accelerated and retarded by Parallax. With an Addendum referring to the possibility of determining the Solar Parallax by the same sort of Observations in* 1874 *as were made in* 1769. By Richard A. Proctor, B.A.

Having applied geometrical tests to the elements discussed in the paper and maps by the Astronomer Royal (*Monthly Notices*, December 11, 1868), I was led to suspect that some of the corrections which would come to be applied in an exact calculation of the circumstances of the two Transits of *Venus* would be larger than a first view of the subject might suggest I now present some of the results to which my examination of the Transit of 1874 has led me, reserving for a future occasion the examination of the Transit of 1882.

I premise that internal contacts are the phenomena to be specially considered; so that, although I have calculated the position angles for external contacts and passages of *Venus's* centre, I have not thought it necessary to determine the exact points on the Earth's surface at which these phenomena are most affected by parallax. I must also add that I have taken as the basis of my calculation the following elements calculated by Mr. Hind, and obligingly furnished me by the Astronomer Royal.

Transit of *Venus*, December 8th, 1874.

For the centre of the Earth.

Ingress.

	h	m	s		
External contact	13	46	56	Greenwich Mean Solar Time.	
Internal contact	14	15	57	,,	,,

Egress.

Internal contact	17	57	5	,,	,,
External contact	18	26	5	,,	,,

The elements sent me also contained the position-angles for external contacts, but as these were only given to tenths of degrees, I preferred to recalculate them.

From an examination of these elements I learn that the estimated diameter of *Venus* is about 7364 miles, corresponding to an apparent semidiameter of $8''{\cdot}305$ at the Earth's mean distance, and an assumed equatorial horizontal solar parallax of $8''{\cdot}94$.

The Sun's longitude on December 8, 1874, at 16^h, I estimate at $256^\circ\ 57'$ (with sufficient approximation); the apparent inclination of the Sun's ecliptical diameter to a declination-parallel $5^\circ\ 36'$ (the western end raised towards the north); the Sun's declination $22^\circ\ 49'$ S.

The formulæ I have used are founded on the following considerations. Conceive the Sun and Earth enclosed in a cone whose vertex is beyond the Earth, and also in a double cone whose vertex lies between the Earth and the Sun. These cones will have a common axis,—namely, the Earth's radius vector. Now, it is obvious that when *Venus* has reached the surface of the outer cone, a transit commences at a certain point on the Earth's surface. As soon as *Venus* has reached the surface of the inner cone, transit has begun for all places on the Earth's illuminated hemisphere. The former contact corresponds to the case of ingress accelerated by parallax, the latter to the case of ingress retarded by parallax. Similar considerations apply to egress as affected by parallax.

It is clear that if we suppose the Earth's radius-vector reduced to rest and the motion of *Venus* correspondingly retarded, the remaining motion of *Venus* is that with which she may be assumed to traverse the section of the pair of cones above described. Thus it is easy to calculate the duration of a central passage for external and internal contacts, or for the centre of *Venus*, and either for the outer or inner cone, or for a cone between the two having the Earth's centre for vertex. The case of external contacts for the last-mentioned cone gives the duration of a central transit as seen from the Earth's centre; and by comparing this duration with that given in Mr. Hind's

elements, we can tell how far the transit of 1874 differs from a central one.

On such considerations the following formulæ and calculations are founded :—

Let D = Sun's diameter.
d = Earth's mean distance.
d' = *Venus's* mean distance.
r = Earth's actual distance.
r' = *Venus's* actual distance.
v = Earth's mean velocity.
v' = *Venus's* mean velocity.
Δ = Earth's mean diameter.
Δ' = *Venus's* mean diameter.
i = inclination of *Venus's* orbit.

Then $v' = v\sqrt{\frac{d}{d'}}$; the actual velocity of the Earth is $v\frac{d}{r}$ (very nearly); that of *Venus* is $v\sqrt{\frac{d}{d'}}\cdot\frac{d'}{r'} = v\frac{\sqrt{d\,d'}}{r'}$; velocity of the Earth's radius vector at *Venus's* distance is $v\frac{d\,r'}{r^2}$. Whence it readily follows that the actual velocity with which *Venus* crosses the section of the cones described above is

$$v = \sqrt{\left\{\frac{\sqrt{d\,d'}}{r'} - \frac{d\,r'}{r^2}\cos i\right\}^2 + \left\{\frac{d\,r'}{r^2}\sin i\right\}^2}$$

$$v = \sqrt{(d^2 r'^4 + d\,d' r'^4 - 2\,d^{\frac{3}{2}} d'^{\frac{1}{2}} r^2 r'^2 \cos i)} + r^2 r'$$

$$= V, \text{ suppose.}$$

Also the cross-section of the cone whose vertex is at the Earth's centre is $D\left(\frac{r-r'}{r}\right)$. Therefore, the duration of a central transit of *Venus's* centre

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r}\right\};$$

the duration of a central transit for external contacts

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r} + \Delta'\right\};$$

the duration of a central transit for internal contacts

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r} - \Delta'\right\};$$

all these phenomena being supposed to be seen from the Earth's

centre. To determine the interval between phenomena affected by parallax, we have, the section of the outer cone mentioned above

$$= \frac{D\,(r-r')}{r} + \frac{\Delta\, r'}{r};$$

the section of the inner cone mentioned above

$$= \frac{D\,(r-r')}{r} - \frac{\Delta\, r'}{r};$$

and it is readily seen that, for a central transit, the interval between first external contact most accelerated by parallax and last external contact most retarded, is

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r'} + \frac{\Delta\, r'}{r} + \Delta'\right\};$$

the interval between first external contact most retarded by parallax and last external contact most accelerated, is

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r} - \frac{\Delta\, r'}{r} + \Delta'\right\};$$

the interval between first internal contact most accelerated by parallax and last internal contact most retarded, is

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r} + \frac{\Delta' r'}{r} - \Delta'\right\};$$

and, lastly, the interval between first internal contact most retarded by parallax and last internal contact most accelerated, is

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r} - \frac{\Delta\, r'}{r} - \Delta'\right\}.$$

By taking the recognised values of D, Δ, Δ', v, d, d', and i; $r = 0.98480$ (the Earth's mean distance being unity), and $r' = 0.72040$, all the above intervals are readily calculated. The duration of a central transit of *Venus's* centre, seen from the Earth's centre, is found to be $7^h\ 55^m\ 8{\cdot}256^s$. A similar transit calculated for external contacts has a duration of $8^h\ 10^m\ 25^s{\cdot}140$, or 8·17365 hours. Since the duration of the transit of 1874, estimated for external contacts is 4·6525 hours, we have for determining the arc (2θ) of which the actual path of *Venus* is the chord,* the equation

* The arc and chord here referred to belong to a circle having a diameter $\left\{D\,\frac{(r-r')}{r} + \Delta'\right\}$; but the position-angles eventually deduced may, of course be referred to the Sun's disk.

$$\sin \theta = \frac{4{\cdot}6525}{8{\cdot}1736} = \sin 34^\circ\ 41'\ 40''\ \text{approximately};$$

and it is clear the distance separating the chord of transit from the centre of the section is

$$= \frac{1}{2}\left\{\frac{D\,(r-r')}{r} + \Delta'\right\}\ \cos\theta.$$

This distance is a constant in the calculation, and it will be convenient to call it k.

Now, if we want to determine the duration (t) of the transit of *Venus* across a section of any other radius, as s, and the corresponding arc of transit ($2\,\varphi$) we have the formulæ

$$\cos\varphi = \frac{k}{s};\ \text{and}\ t = \frac{2\,s\sin\varphi}{V}$$

We must give to $2\,s$, successively, the four values included in the expression

$$\left\{\frac{D\,(r-r')}{r} \pm \Delta\,\frac{r'}{r} \pm \Delta'\right\}.$$

In this way the durations were calculated which correspond to the epochs given in the accompanying table. For deducing position-angles from the arcs of transit, we have

The apparent angle at which *Venus* is ascending from the ecliptic towards the west

$$= i + \tan^{-1}\left\{\frac{d\,r'^2\sin i}{\sqrt{d\,d'}\,r^2 - d\,r'^2\cos i}\right\} = 9^\circ\ 9' +$$

The angle at which the elliptical diameter of the Sun is inclined to a declination-parallel (west end raised towards the north)

$$= 5^\circ\ 36'.$$

Therefore the apparent angle at which *Venus* is crossing a declination-parallel, rising towards the west, $= 14^\circ\ 45'$.

Thence it readily follows that the arc of transit $2\ (34^\circ\ 41'\ 40'')$ corresponds to the following position-angles,—

External contact at ingress, 130° 33′ from S. through E. towards N.
„ „ egress, 160° 4′ „ „ W. „

These values correspond satisfactorily with those given by Mr. Hind, viz. 130°·6 and 160°·0. The same angle 14° 45′ applied to the transit-arcs obtained in the other cases examined, gives the position-angles in the second column of the following table:—

Elements of the Transit of Venus, December 8th, 1874.

	G. M.T. h m s	Position angle. ° ′	
First external contact, most accel. by par.	13 37 35	128 48	from S. to E.
,, ,, from Earth's centre	13 46 56	130 33	,,
,, ,, most retarded by par.	13 57 47	132 41	,,
First internal contact, most accel. by par.*	14 3 59	133 56	,,
,, ,, from Earth's centre	14 15 57	136 28	,,
,, ,, most retarded by par.*	14 29 5	139 26	,,
Last internal contact, most accel. by par.*	17 43 58	168 57	from S. to W.
,, ,, from Earth's centre	17 57 5	165 59	,,
,, ,, most retarded by par.*	18 9 4	163 27	,,
Last external contact, most accel. by par.	18 15 15	162 12	,,
,, ,, from Earth's centre	18 26 5	160 4	,,
,, ,, most retarded by par.	18 35 26	158 19	,,

To which may be added,

First passage of ♀'s centre across Sun's limb	14 0 51	133 17	from S. to E.
Last ,, ,, ,,	18 12 11	162 48	from S. to W.

(both as seen from the Earth's centre).

The phenomena marked * are those which now concern us. We notice, first, that the maximum interval of time to be determined by the observations applied to the ingress and egress (internal contacts) is $25^m\ 6^s$. This is the interval which would result if each observation were made when the Sun is only raised above the horizon through the effects of refraction. A somewhat longer interval might be dealt with if the effects of refraction were considered, and if the observation of contact were possible when the Sun is close to the horizon. But we need not inquire into the possible extent by which the interval might thus be increased, since observations when the Sun is on the horizon are not available in the present instance. Practically we may consider that an interval of about 20^m has to be dealt with, or at the outside 22^m. As this interval is directly proportional to the solar parallax we can estimate the effect of errors of observation, or of error in the determination of the longitude of a station. An error of one second in the determination of an interval of 21^m would correspond to an error of about $0''{\cdot}007$ in the determination of the solar parallax.

(In an addendum to this Note, I propose to deal with the method of determining the solar parallax by difference of durations, so far as this method is applicable to the transit of 1874. Unless I mistake, I shall be able to show that more is to be hoped from the latter method, than from the one which is the object of the present investigation.)

To return to the consideration of the actual circumstances under which ingress and egress are most affected by parallax.

To determine the longitude and latitude of the four points on the Earth's surface at which the acceleration and retardation have their maximum values, we have the following formulæ—

Let P = the position-angle, measured from the south point.

δ = Sun's declination.

e = equation of time (additive to mean time).

h = hour at which a phenomenon (ingress or egress) occurs.

l = latitude of point required.

λ' = longitude of point required (l' being estimated from the meridian along which it is apparent noon at the epoch of the phenomenon).

Then $\sin l = \cos P \cos \delta$; and $\cot \lambda' = \cot P \sin \delta$.

I give the formulæ in this form as being that I have made use of. Strictly speaking, a reference should be made to the signs of l, λ'; but the circumstances of each case are quite sufficient to clear up any doubts which might arise. It is obvious that for the first and fourth phenomena marked * in the above table, the latitude is north, and for the others south; because, referring to the preliminary considerations it is seen that the tangent-lines corresponding to the former pair of phenomena belong to a cone whose vertex is outside the Earth, whereas the tangent-lines corresponding to the other phenomena belong to a cone whose vertex lies between the Earth and the Sun. From similar considerations it follows that the longitudes in the first and third cases are eastward of the meridian along which it is noon at the respective epochs, whereas, in the second and fourth cases, the longitudes are westward of the corresponding meridian. With this relation in view, the actual latitude (λ) east of Greenwich is given by the expression

$$\lambda = 24^h - (h + e) \pm \lambda'$$

(*Note.* e changes by a few seconds during the progress of the transit.)

Thus we obtain the following results:—

	Lat.	Long.
(i) The place at which first internal contact is most accelerated lies in	39° 45′ N.	143° 23′ W.
(ii) The place at which first internal contact is most retarded lies in	54 27 S.	29 1 E.
(iii) The place at which last internal contact is most accelerated lies in	64 47 S.	114 22 W.
(iv) The place at which last internal contact is most retarded lies in	62 5 N.	48 30 E.

On a comparison of these positions with those marked in Mr. Airy's maps I., II., III., IV., it will be noticed that the agreement is much closer for the places which fall in north latitude (that is,

cases I. and IV.) than for the other two. Indeed, as respects the two former positions there is little to affect the general conclusions which the Astronomer Royal has deduced from geometrical considerations. In the case illustrated by Plate I. the point marked with a small circle should be removed some 280 miles towards the north-west, rather westerly, and in the case illustrated by Plate IV. the corresponding point should be removed some 220 miles towards the north-east.

It is in the cases illustrated by Plates II. and III. that the most important changes have to be made. In the first the point marked with a circle should be removed to a distance of upwards of 800 miles towards the south-west. This change is unfavourable so far as the choice of stations for observing the retarded egress is concerned. Fort Dauphin in the Island of Madagascar (where, however, the Sun would be low at the time of internal contact at ingress) and Prince Edward and Marion Islands, are the places which, with Crozet's Island and Kerguelen's Land, are best placed for this observation.

In the case illustrated by Plate III. the change which should be made is not quite so large. The point marked with a circle should be shifted nearly 700 miles towards the south-east, rather easterly. This change is also unfavourable, as it removes the point further away from all the places mentioned by the Astronomer Royal. It follows that Auckland Island, Emerald Island, and Victoria Land, are the places best situated for observing the egress accelerated by parallax.

A portion of the corrections which have here been mentioned are due to the choice of internal contacts as the phenomena to be chiefly attended to. I believe no doubt can exist that this choice is a just and proper one. Indeed, so far as I am aware, no calculations for the determination of solar parallax have ever been founded on attempts to determine the epoch of external contacts.

In a subsequent paper I propose to present the results of calculations similar to the above, applied to the transit of 1882. I believe, however, that the corrections which have to be made in the case of that transit are less considerable than those dealt with above.

Addendum on the possibility of Determining the Solar Parallax by observations applied to the Transit of 1874 *of the same sort as those applied to the Transit of* 1769.

It occurred to me, while considering the relations presented in the above paper that there might possibly be some little inaccuracy in the Astronomer Royal's view that the transit of 1874 is altogether unfit for the determination of the difference between the Sun's parallax and the parallax of *Venus* by the method on which reliance was exclusively placed in the treatment of the observations of 1769. I have not seen the papers of May 8, 1857, and June 10, 1864, in which this view is presented; but I am told

that Lesson XLVI. of Mr. Lockyer's *Elementary Astronomy* is derived from the Astronomer Royal's papers. I conclude, therefore, that the consideration on which the view is founded is the following:—To determine the greatest difference in the length of the chords of transit, we must, if possible, select a place where both the ingress will be accelerated and the egress retarded, and another where the ingress is retarded and the egress accelerated. And I assume that the impossibility of finding two places which sufficiently fulfil these conditions in the case of the transit of 1874 is held to involve the failure of the method founded on the observed durations of transit.

It is unquestionably the case that no place can be found in northern latitudes which is at once near the point where ingress is most accelerated, and to the point where egress is most retarded. Nor can any spot be found near to the points where ingress is most retarded and egress most accelerated.

But this circumstance is counterbalanced by one which operates in another direction. In the case of the transit of 1874, the absolute maximum duration of transit, that is, the duration counted from ingress most accelerated to egress most retarded *for the whole earth*, differs very much more from the absolute minimum than in the case of the transit of 1882. For internal contacts the former difference exceeds the latter in the proportion of about three to two. This arises from the comparative shortness of the chord of transit in the case of the transit of 1874, and the increased effect which parallax consequently produces on its apparent length.

Hence it follows that although we cannot use anything like the full value of the difference in the case of the transit of 1874, we can yet obtain a resulting actual difference which is *fully equal* to that which we can obtain from the transit of 1882.

In order to test this view, I have made use of the following reductions for the effects of parallax, supplied with Mr. Hind's elements of the two transits:—

For the transit of *Venus* in 1874, the reductions for any place (taking ρ = Earth's radius at the place; l = north latitude; and λ = latitude east of Greenwich), are

For Ingress.

External contact $-[2\cdot5621]\,\rho \sin l - [2\cdot6872] \cos l \cos(\lambda - 137^\circ\ 1').$

For Egress.

External contact $+[2\cdot7213]\,\rho \sin l + [2\cdot4833] \cos l \cos(\lambda - 38^\circ\ 20').$

In the case of the transit of 1882, the corresponding reductions are,—

For Ingress.

External contact $+[2\cdot5332]\,\rho \sin l - [2\cdot4657]\,\rho \cos l \cos(\lambda - 87^\circ\ 55').$

For Egress.

External contact $-[2\cdot2261]\,\rho \sin l + [2\cdot6203]\,\rho \cos l \cos(\lambda - 135^\circ\ 0').$

I find that, if we take for the northern station in 1874, a place on the continent of Asia about 45° north latitude, and 120° east longitude; and for the southern station a place on Sabrina Land in 66° south latitude and 135° east longitude, there results a difference of duration of transit, equal to about $23^m\ 23^s$. By taking the southern station in about the same latitude and 105° east longitude (the place pointed out by the Astronomer Royal as suitable for the southern station in 1882), the difference becomes diminished to $22^m\ 44^s$. By taking Crozet's Island or Kerguelen's Land for the southern station the difference still continues large, though somewhat diminished. But by taking a place somewhat to the south-west of Nicolaief (close to Saghalien Island) for the northern station, and a place in 66° south latitude, and 135° east longitude for the southern station, the difference is increased to no less than $26^m\ 30^s$ (at the northern station the Sun would be low at the time of external contact at egress). The southern station might also be taken at Enderby Land (66° south latitude, and 50° east) without reducing the difference more than one minute.

Now in the case of the transit of 1882 the places selected by the Astronomer Royal, as being absolutely the most favourable so far as difference of duration is concerned, are, the Island of Bermuda and Sabrina Land (in longitude 105° east); and these places give a total difference of duration of $25^m\ 49^s$, nearly 41″ *less* than the greatest difference obtained above for the transit of 1874.

It will be seen also that the method founded on observed differences in the duration of transit has altogether the advantage over the other method; since we have a larger period of time to deal with by some minutes, and there is not in the case of the former method the same necessity for a scrupulously exact determination of the longitude of the place of observation.

Since the above was written I have re-examined the whole subject by a new method, with results strictly accordant with those above obtained. But additional inquiry into the circumstances of the transit of 1874, with special reference to the question of duration, and for the case of internal contacts, has led me to still clearer evidence of the value of this transit. I have constructed the orthographic projections of the Earth for the epochs of first and last internal contacts as seen from the Earth's centre. Across these projections I have laid down the lines which mark the order of the occurrence of internal contacts as seen from different parts of the Earth's surface, from minute to minute. Thus, in the first projection I have a series of twenty-six lines commencing with a tangent to the Earth's north-easterly quadrant, 133° 56′ from the north, and ending with a tangent to the Earth's south-westerly quadrant, 139° 26′ from the south: in the second I have a similar number of lines commencing with a tangent to the Earth's south-easterly quadrant 165° 59′ from

the north, and ending with a tangent to the Earth's north-westerly quadrant 160° 4′ from the south. These lines enable one at once to recognise the best stations for observing the transit as most lengthened and as most shortened by parallax.

I find that any part of a nearly circular region extending from Lake Baikal to the southern part of Saghalien Island, and from north latitude 40° to north latitude 60° gives a well-lengthened transit, while the northernmost part of this region gives a lengthening of about $16\frac{1}{4}^m$. For southern stations there is a wide range of choice, though only three or four stations give nearly the full available shortening. I find, that Petra Island (assuming it rightly placed in my atlas, viz. in west longitude 88°, and south latitude 71°), gives a transit shortened by 20^m. The place near Repulse Bay mentioned by the Astronomer Royal as suitable for the observation of the transit of 1882, gives in 1874 a shortening of $18\frac{1}{2}^m$. The place near Victoria Land similarly referred to by Mr. Airy, gives a shortening of $18\frac{1}{4}^m$ in 1874. A place on Enderby Land will give a shortening of no less than $20\frac{1}{4}^m$. Crozet's Island and Kerguelen's Land are also suitable, giving transits shortened by 17^m and 16^m respectively. Macquarie Island, Royal Company Island, Hobart Town, and even parts of New Zealand, would serve as useful subsidiary stations.

The best southern station taken in conjunction with the best northern station gives a difference of duration of no less than $36\frac{1}{2}^m$. The maximum of difference in 1882 will be about 28^m. This is founded on the Astronomer Royal's statement that Sabrina Land and Bermuda give a difference which is to the absolute maximum of difference ($32\frac{3}{4}^m$ for internal contacts) as 341 to 400. Thus if we assume the value of a transit to be directly proportional to the observable difference of duration, the transit of 1874 is more valuable than that of 1882 in the proportion of $36\frac{1}{2}$ to 28, or more than 9 to 7. If, however, we hold that the slowness of ingress diminishes the value of a transit in precisely the same proportion that it increases the observable difference of duration, the transit of 1874 is inferior to that of 1882 in the proportion of

$$\frac{36\frac{1}{2}}{50\frac{1}{2}} \text{ to } \frac{28}{32\frac{1}{4}} \text{ or almost exactly 6 to 7.}$$

Probably the truth lies between these results, so that we may assign to the transit of 1874 a value which bears to that of 1882 the ratio of

$$\tfrac{1}{2}(9+6) \text{ to } 7, \text{ that is, } 15 \text{ to } 14.$$

Judged in either way the transit of 1874 is superior in value to that of 1769, *as actually utilized.*

In any case, it seems clear that the method founded on observed differences of duration is not inapplicable to the transit of 1874. On the contrary, when we remember all the difficulties in the way of the second method, the former seems to

be the one from which the most satisfactory results are to be expected.

Mr. Stone has obligingly called my attention to papers in the *Comptes Rendus* by M. Puiseux, in which the duration of transit has been calculated for 9 places in 1874, with the Sun's elevation at the epoch of each phenomenon. I have not yet had an opportunity of examining this paper, or noting how far its author's object has been similar to that which I have had in the discussion of the results contained in the latter part of the foregoing paper.

Limits of Total Phase of the Solar Eclipse, 1869, August 7.
By J. R. Hind, Esq.

From $8^h\ 51^m$ to $11^h\ 9^m$ G.M.T.

G.M.T.	Northern Limit. Long.	Northern Limit. Lat.	Central Line. Long.	Central Line. Lat.	Southern Limit. Long.	Southern Limit. Lat.	Dur. of Total Phase on Cen. Line.
h m	° ′	° ′	° ′	° ′	° ′	° ′	m s
8 51	235 41 E.	59 7 N.	140 12 E.	59 2 N.	144 1 E.	58 47 N.	2 35
8 53	140 59	60 22	144 53	60 3	148 20	59 39	2 41
8 55	145 31	61 20	149 4	60 53	152 14	60 21	2 46
8 57	149 38	62 8	152 55	61 34	155 52	60 56	2 51
8 59	153 29	62 47	156 32	62 7	159 17	61 25	2 55
9 1	157 9	63 20	159 59	62 36	162 31	61 50	2 59
9 3	160 39	63 48	163 17	63 0	165 38	62 10	3 2
9 7	167 15	64 31	169 31	63 36	171 33	62 40	3 10
9 11	173 26	65 0	175 23 E.	64 0	177 5 E.	63 0	3 16
9 15	179 17 E.	65 16	179 7 W.	64 12	177 44 W.	63 9	3 21
9 19	175 10 W.	65 22	173 54	64 15	172 48	63 10	3 26
9 23	169 55	65 19	168 58	64 10	168 8	63 3	3 30
9 27	164 55	65 9	164 16	63 59	163 42	62 51	3 34
9 31	160 12	64 50	159 49	63 41	159 30	62 32	3 38
9 35	155 44	64 27	155 37	63 17	155 31	62 8	3 41
9 39	151 31	63 58	151 37	62 48	151 44	61 40	4 43
9 43	147 32	63 24	147 51	62 15	148 8	61 8	3 45
9 47	143 46	62 47	144 16	61 39	144 43	60 32	3 47
9 51	140 12	62 5	140 51	60 59	141 28	59 53	3 48
9 55	136 49	61 20	137 37	60 15	138 22	59 11	3 48
9 59	133 36	60 32	134 32	59 28	135 24	58 26	3 49
10 3	130 33	59 41	131 35	58 39	132 33	57 38	3 49
10 7	127 38	58 47	128 46	57 47	129 49	56 48	3 49
10 11	124 50	57 50	126 3	56 53	127 11	55 55	3 48
10 15	122 7	56 52	123 25	55 56	124 38	55 1	3 47
10 19	119 30	55 51	120 52	54 57	122 9	54 4	3 45

From $8^h\ 51^m$ to $11^h\ 9^m$ G.M.T.

G.M.T.	Northern limit. Long.	Lat.	Central Line. Long.	Lat.	Southern Limit. Long.	Lat.	Dur. of Total Phase on Cen. Line.
h m	° ′	° ′	° ′	° ′	° ′	° ′	h m
10 23	116 57	54 48	118 22	53 56	119 42	53 5	3 43
10 27	114 27	53 42	115 55	52 53	117 18	52 4	3 41
10 31	111 58	52 34	113 30	51 47	114 56	51 0	3 38
10 35	109 31	51 23	111 5	50 39	112 34	49 54	3 35
10 39	107 2	50 9	108 40	49 27	110 10	48 45	3 31
10 43	104 32	48 53	106 12	48 13	107 46	47 33	3 27
10 47	101 57	47 32	103 40	46 56	105 18	46 19	3 22
10 51	99 15	46 8	101 2	45 34	102 42	45 0	3 16
10 55	96 23	44 39	98 13	44 8	99 58	43 37	3 10
10 59	93 14	43 2	95 12	42 36	97 3	42 9	3 4
11 1	91 31	42 11	93 34	41 27	95 28	41 22	3 0
11 3	89 41	41 16	91 49	40 55	93 48	40 32	2 56
11 5	87 40	40 18	89 56	40 1	92 0	39 40	2 52
11 7	85 26	39 16	87 51	39 3	90 2	38 45	2 48
11 9	82 52 W.	38 6 N.	85 31 W.	37 59 N.	87 52 W.	37 46 N.	2 44

Nautical Almanac Office,
1869, *March* 1.

Occultations of Stars observed at Waterloo, near Liverpool.
By John Joynson, F.R.A.S.

1868.			G.M.T. h m s	Nautical Almanac. h m	Angle given. °		
Dec. 27	B.A.C. 1526	Reap.	7 25 36·0	7 22	318	± 3^m	later, as it should be.
1869. Jan. 23	Aldebaran	Dis.	8 44 15·8	8 49	97	± 5^m	earlier; should be later.
		Reap.	±10 0 0	10 5	289	± 5^m	earlier; should be later.
24	115 Tauri	Dis.	5 27 5·0	5 29	52	± 2^m	earlier, as it should be.
		Reap.	6 16 38·6	6 12	333	± 3^m	later ,,
	119 Tauri	Dis.	8 17 25·7	8 14	143	± 3^m	later ,,
		Reap.	9 0 44·6	9 10	237	± 9^m	earlier ,, but hardly so much.
	120 Tauri	Dis.	8 46 27·4	8 48	117	± 2^m	earlier, should be later.
		Reap.	9 48 13·3	10 0	260	± 12^m	earlier, as it should be, but hardly so much as 12^m ?

Lat. 53° 28′ 24″ N.
Long. $0^h\ 12^m\ 7^s$ W.

The reappearance of *a Tauri* (*Aldebaran*) could not be got exactly for passing cloud; but it was seen at $10^h\ 1^m$ G.M.T. about 75 seconds of arc from the limb; so that it must have reappeared rather before 10 P.M.

The times got on the 24th were deemed exact in each case, though in some cases they differ more than I think they should from the times stated in the *Nautical Almanac*.

Waterloo, Feb. 5, 1869.

Note on Personality in the Determination of the Line of Collimation of a Transit. By E. J. Stone, Esq.

A personality has been noticed between some observers in the determination of the line of collimation of a Transit instrument. The existence of such a personality to a sensible amount would be extremely difficult to deal with in a perfectly satisfactory manner: it would cast a certain amount of doubt over the whole of the instrumental corrections. I have thought it worth while, therefore, to determine whether such a personality exists between the Greenwich observers. The observations at Greenwich are chiefly made by Messrs. Dunkin, Ellis, Creswick, and James Carpenter. The collimation error of the Transit-circle is determined on every day except Sundays. It is usual, unless there are indications of a real change, to adopt the mean of the readings made during a week as the true reading for the line of collimation to be used in reduction of the observations made during the week. We shall have, therefore, in many cases, a collimation reading made by Messrs. Dunkin, Ellis, Creswick, and Carpenter, all within a week, and generally two collimation determinations by one or other of these gentlemen within the week. These duplicate determinations are valuable for testing the stability of the line of collimation during the week. I have adopted the mean of such duplicate determination for the observers' reading for the week's comparison. In the last two years I have found 38 of these complete weekly comparisons. These 38 comparisons, split up into three sections of 11, 16, and 11 comparisons, give results so closely agreeing with the means of all, that I have not thought it necessary to extend the comparisons.

The following are the corrections to the adopted readings, as determined by Messrs. Dunkin, Creswick, Ellis, and Carpenter:—

Correction from Mr. Dunkin's Observations		$+0^{r}.021$
Mr. Ellis'	,,	$+0^{r}.024$
Mr. Creswick's	,,	$+0^{r}.014$
Mr. Carpenter's	,,	$+0^{r}.018$

or, taking the deviation from the mean as error of each observers' method, we have





John Browning del.

TRAIN OF SUN-SPOTS.

Fig. 1.

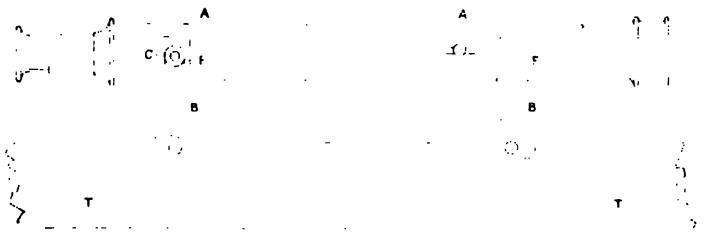


Fig. 2.

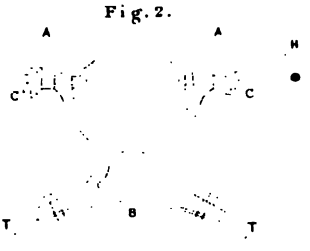


Fig. 4.

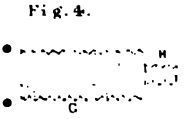


Fig. 3.



NEW MOUNTING FOR FINDER.

John Browning del.

Malby & Sons, Lith.

For Mr. Dunkin	$+0^{s}\cdot002$	=	$0''\cdot030$
Mr. Ellis	$+0^{s}\cdot005$	=	$0''\cdot075$
Mr. Creswick	$-0^{s}\cdot005$	=	$-0''\cdot075$
Mr. Carpenter	$-0^{s}\cdot001$	=	$-0''\cdot015$

The greatest difference is between Mr. Ellis' and Mr. Creswick's determinations, but these differ by only $0''\cdot15$.

This quantity, small as it really is, is real. Each of the three sub-groups gives a result of the same sign, agreeing closely in amount, and an independent comparison of 51 consecutive readings of C. and E. gives a similar result. The magnitudes of these discordances are so small, that they may fortunately be most safely neglected. The result appears to be, that, so far as Greenwich is concerned, the uncertainties introduced into the collimation determinations by personality are insignificant.

On an extensive Train of Sun-spots. By John Browning, Esq.

About noon, on the 7th of March, I saw a long train of Sun-spots through the finder of my Equatoreal.

Attempting to observe them with my 12-inch silvered-glass reflector, I found the definition so bad that I was compelled to reduce the aperture to 6 inches before I could obtain tolerably steady vision. I then fitted a solar plane, that is a plate of parallel glass silvered on the exposed surface, on to the mouth of the telescope.

With a parallel-wire micrometer I measured the spots, and made the accompanying drawing to scale, see Plate.

They presented the appearance of an almost continuous penumbra, in an irregular hollow curved line, with umbræ at intervals. Some of these umbræ contained blacker nuclei.

On the western side a portion of the penumbra assumed the form of a pair of compasses.

I found the outside dimensions of the cluster to be from E. to W. 97,700 miles, and from N. to S. 27,130 miles. This is by far the largest group of spots I have seen during the recent outbreak. Taking a mean line through the group, they were almost exactly parallel to the Sun's equator.

A few bright bridges crossed some of the penumbræ, but no fine stipplings such as I have mostly seen were visible on them. The air was, however, so unsteady that I could only use a power of 127. A higher power than this would probably have been necessary to make them perceptible. The coarse mottlings were easily seen all over the solar surface.

In the case of some of the previous spots I observed that when they were near the centre of the solar disk but few bright faculæ could be seen surrounding the penumbra. As these spots

approached closely to the Sun's limb the faculous matter became brighter and brighter, until when the spots had reached the limb it quite obliterated the penumbra. The black spots were then seen surrounded by a bright border. This is a proof, if any is needed, that the faculæ are far above the solar surface.

On an Improved Method of Mounting Finders.

By John Browning, Esq.

The manner of mounting a finder may seem too small a matter to bring before the Royal Astronomical Society, yet when it is considered that a valuable observation may be lost merely from a finder being out of adjustment, it will at once be admitted that the matter is one well worthy of attention.

Professor Piazzi Smyth wrote me a short time since "that he thought some better plan for mounting and adjusting finders was much required." This induced me to devise the contrivance I shall presently describe.

Two plans are in general use for adjusting finders, both of which have obvious disadvantages. In the first the finder is placed in the centre of two rings somewhat larger than itself, and adjusted by means of three screws working through each ring, and pressing on bands which serve to strengthen the finder. This adjustment is liable to the following objections:—The rings prevent a line of sight being taken along the body of the finder; the screw-points are apt to shift their position on the rings, even if resting in groves, should the observer inadvertently attempt to direct the telescope by means of the finder. Like all three-screw adjustments, several trials have generally to be made before an error can be rectified.

The second plan consists in attaching the finder to the body of the telescope as a fixture, on two uprights. The adjustment in this case is made by moving a stop carrying the cross-wires placed in the focus of the eye-piece, by means of screws which pass through the body of the finder. In most respects this plan is good, but it has the serious objection of considerably limiting the field of view. Of course the largest attainable field of view is required in a finder.

The plan figured in the Plate seems to me to avoid the whole of the foregoing objections.

Fig. 1 represents the finder (A A) mounted by the aid of the uprights (B B) on the telescope (T T). The finder has two semicircular bands (F F) round the lower half of the body. The upright (B) has two hollow gun-metal screws (C C), which work in bosses at the sides. These screws press against the stout semicircular band (F), and serve to move the finder horizontally. Two smaller steel screws pass through the gun-metal screws, and are themselves screwed into the band (F) on the finder. The con-

struction of these double screws is shown in fig. 4, where G is the hollow screw, the end of which acts against the band, and (H) the central screw which clamps into the finder.

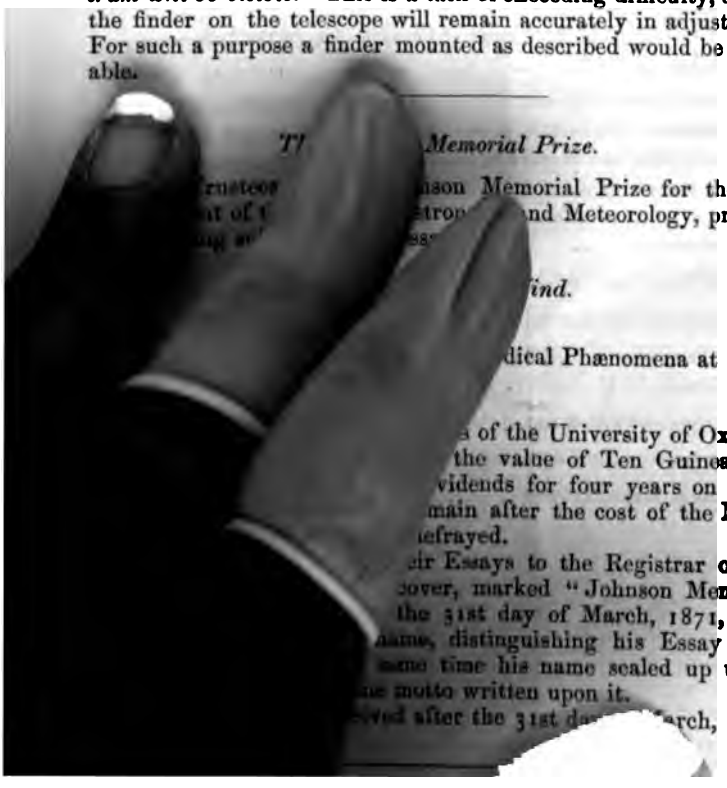
Fig. 2 is a section of the finder and upright, as well as the clamping and adjusting screws, at the point B.

The vertical adjustment is effected by means of a similar pair of screws, as at D, fig. 3.

It will be evident from the construction that when the finder is being moved vertically it will swivel slightly on the horizontal screws, C C, while, when it is being moved horizontally, it will swivel on the vertical clamping screw, D, fig. 3; thus no undue strain will be thrown on any part.

I have the honour of exhibiting a 2-foot finder mounted in this manner, and I think a trial of its firmness will convince any one that it would not be possible, except by breaking or bending some of its parts, to put it out of adjustment.

When using a star-spectroscope attached to a telescope it is necessary to bring the star or other object under examination exactly between the jaws of the slit of the spectroscope, *or no spectrum will be visible.* This is a task of exceeding difficulty, unless the finder on the telescope will remain accurately in adjustment. For such a purpose a finder mounted as described would be valuable.


Memorial Prize.
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of the University of Oxford;
the value of Ten Guineas, to-
vidends for four years on 338*l.*
main after the cost of the Medal
efrayed.
ir Essays to the Registrar of the
over, marked "Johnson Memorial
the 31st day of March, 1871, each
name, distinguishing his Essay by a
time his name sealed up under
motto written upon it.
after the 31st d arch, 1871.

less chance of mistake. The experiments of Sir Wm. Herschel as to the views which even unpractised observers form respecting the centering of a disk show how little chance of error there is in a matter of this sort; and in the case of a practised observer like Mr. Browning the chance of error was much diminished.

The special work, then, which I asked him to favour me by carrying out was the observation of a central passage of the Kaiser Sea (using the nomenclature of my chart of *Mars*) — that is, the long funnel-shaped sea running north and south, sometimes called the Hour-glass Sea. This feature is depicted in two views of *Mars* taken by Hooke on March 12th, 1666 (New Style), at $12^h\ 20^m$ and $12^h\ 30^m$; and my former estimates of *Mars*' rotation period had been obtained by comparing Hooke's views with drawings of the same feature by Herschel, Beer and Mädler, Dawes and Browning. From the close agreement between the two drawings taken by Hooke and the true figure given to the Kaiser Sea, I am disposed to place much more reliance on his observations than one would ordinarily accord to the work of the astronomers of the seventeenth century. There can be no doubt that he saw the Kaiser Sea under peculiarly favourable circumstances on that spring night in 1666, since his drawings will bear favourable comparison with any that have been obtained in recent times with instruments of moderate power.

So much for the observation which limits one end of the long period.

The observation next to be considered is the one which Mr. Browning obligingly made at my request. According to this observation the Kaiser Sea was centrally situated on the disk of *Mars* on February 4, at $11^h\ 3^m$ G. M. T. A few minutes before this hour Mr. Browning felt certain that the sea was to the left of the centre, and a few minutes after he felt certain it was to the right. This will be understood when we remember that a point near the middle of the disk traverses $\frac{1}{4\frac{1}{2}0}$th of the disk's diameter in a minute. So that if we take a range of eight minutes on either side of the epoch of central passage, we get a range of motion equivalent to about $\frac{1}{3\frac{1}{2}}$th part of the disk's diameter, and there would be no mistaking so considerable a motion as this.

At the hour of observation the longitude of the Earth was about $135\frac{1}{2}$ degrees; that of *Mars* about $141\frac{1}{2}$ degrees. Marking down the points thus indicated upon my chart of the orbits of the four inner planets, and drawing in also the points occupied by the two planets at the epoch of Hooke's observation, I found the difference of *Mars*' geocentric longitude to be about $35\frac{1}{2}$ degrees. From the care taken in the construction I think this result may be trusted within a quarter of a degree at the outside. It results that the correction for geocentric longitude may be obtained by adding the period in which the planet rotates through an angle of $35\frac{1}{2}$ degrees, or by subtracting the period of rotation through $324\frac{1}{2}$ degrees.

Next, as to the correction for phase. It is obvious from Hooke's first drawing—see *Monthly Notices* for January 1868—that the Kaiser Sea was some 18 degrees (or perhaps rather more) from central passage at the hour of observation. Thus we must deduct a further interval equivalent to the period of the planet's rotation through somewhat more than 18 degrees, because Browning observed the planet when rotated so many degrees further than when seen by Hooke.

We have then, in all, a deduction equal to the period of the planet's rotation through $324\frac{1}{2}° + 18\frac{1}{2}°$ (say) $= 343°$. This corresponds to a deduction of

$$\frac{343}{360} \times 88643 \text{ seconds,}$$

or

84457 seconds.

Now the total number of seconds between

March 12th, 1666, $12^h\ 20^m$,

and

February 4th, 1869, $11^h\ 3^m$,

will be found to be

6402926580 seconds,

and the corrected interval is therefore,

(6402926580 − 84457) seconds
= 6402842123 seconds.

The number of rotations obtained by dividing this period by a rough estimate of the rotation-period at 88642·7 seconds is found to be 72232.

Lastly, dividing 6402842123 by 72232, we obtain for the rotation-period the value

88642·736 seconds,

or

$22^h\ 37^m\ 22{\cdot}736$ seconds,

the value named in my last paper on the subject being

$22^h\ 37^m\ 22{\cdot}735$ seconds,

and the limits of probable error

0·005 seconds.

The three intervals before examined, ranging from 190 to 201 years, gave severally

88642·737 seconds
88642·734 ,,
88642·734 ,,

The interval now considered, one of nearly 203 years, gives a value between the greatest of these and the two others; and as all the values now obtained lie between

88642·73 s. and 88642·74 s.

we may assume that the planet's rotation period is fairly represented by the value

$22^h\ 37^m\ 22{\cdot}735$ seconds,

with a probable error of ·005 and a possible error of $\cdot 015^s$. More than this error, which gives a range of $\cdot 03^s$ for each rotation, and therefore $36^m\ 6^s{\cdot}96$ for the total number of rotations since the date of Hooke's observations, it would be unreasonable to admit, since the planet's change of appearance, even in a quarter of an hour, is perfectly cognisable by a practised observer.

Kaiser's period ($22^h\ 37^m\ 22^s{\cdot}62$) which differs from mine by $0^s{\cdot}115$, would throw the planet out, at the period of Hooke's observation, by an amount corresponding to the amount of rotation during $2^h\ 20^m$. This is not to be thought of for a moment. Hooke depicts the Kaiser Sea at a distance of rather more than 18° from the centre, and the change resulting from Kaiser's period would throw the Kaiser Sea nearly 50° from the centre. At this distance it would have been lost in the haze near the planet's limb, even had Hooke's telescope been as powerful as the best modern instruments. On the other hand, the fact that Kaiser was led to the period $22^h\ 37^m\ 22^s{\cdot}62$ by the comparison of some of the best modern drawings, taken over a widely extended period, suffices to prove that Hooke's drawing depicts the Kaiser Sea, and not the Dawes' Strait, which sometimes looks not unlike the Kaiser Sea, especially at that season of the Martial year in which Hooke observed the planet. For Kaiser's period throws the Dawes' Strait almost exactly in the centre of that hemisphere of *Mars* which would have been turned away from Hooke at the time of his observation.

The period deduced by Messrs. Beer and Mädler, $22^h\ 37^m\ 23^s{\cdot}8$, is disproved by Sir W. Herschel's drawings; otherwise it would fairly account for Hooke's drawing on the omission of a complete rotation. Herschel's views coming midway between Hooke's and modern drawings satisfactorily remove any doubt of this sort.

On the Meteoric Shower of November, 1868, as seen at the Royal Observatory, Cape of Good Hope. By Geo. W. H. Maclear, Esq.

I have the honour to forward for the information of the Royal Astronomical Society, the result of the watch kept up at this Observatory, on the 13th, for the November meteoric shower.

The first meteor from *Leo* was noted at $13^h\ 18^m\ 30^s$, Cape mean time.

Shower at its maximum about $14^h\ 42^m$.

Last meteor noted (in broad daylight) at $16^h\ 27^m\ 20^s$.

The total number observed amounted to 134, but a dense haze prevailing over the eastern horizon doubtless obscured many others from view. For the same reason we were unable to observe the exact radiant point. Tracing back some of the brighter ones, it appeared to be between γ and ζ *Leonis*, and a little to the north of γ.

No meteors were noted on the 12th of November.

On the 14th the sky was clouded.

Meteoric Shower of November 13th, 1868.

Cape Mean Time. h m s	Direction. From	To	Remarks.
13 18 30	Leo	Zenith	White, large
27 40	,,	Westward	Orange and greenish, large
34 35	Hydra	E. horizon	Small
36 32	γ Leonis	S.	Faint
49 0	Leo	Zenith	White and green
52 5	,,	S. along horizon	,,
57 50	α Hydræ	Horizon	Small
57 50	,,	,,	,,
13 58 5	γ Leonis	Horizontal to S.	,,
14 1 45	η Leonis	Zenith	
3 5	,,	S.	
6 10	,,	S.W.	
7 45	,,	,,	
11 30	Leo	S.	
12 10	Below Regulus	Horizon	Blue and green
14 30	Regulus	S.	Faint
15 20	,,	,,	,,
15 50	,,	,,	,,
17 20	Above Regulus	Horizon	,,
18 35	Leo	S.W.	Orange, large
20 27	,,	Horizontal to S.	
22 20	,,	,,	Small, no train
22 32	,,	,,	Large
23 10	,,	Zenith	,,

Cape Mean Time. h m s	Direction. From	To	Remarks.
14 23 33	Leo	S.	
24 47	Below Leo	Horizontal to S.	Small
25 0	,,	,,	
25 40	,,	,,	
26 50	,,	,,	
28 10	,,	,,	
30 58	,,	,,	
32 45	,,	,,	
33 55	,,	,,	
34 15	Below ♂	Horizon	
35 30	Regulus	S.W.	Orange, large
36 5	μ Leonis	Horizon	Blue, "pear" shaped
36 30	,,	S.	
36 50	,,	,,	
37 5	,,	,,	
37 24	Regulus	,,	Long train
40 35	Below Regulus	,,	Small
41 50	Leo	,,	
42 0	,,	,,	
42 3	,,	,,	
42 12	,,	Horizontal to S.	
42 12	,,	,,	
42 13	,,	Zenith	
43 30	,,	Horizon	
44 20	S. of Regulus	S.W.	Head, orange; train, blue an white. Very fine
45 57	Leo	Horizontal to S.	Bright
46 32	,,	S.	Orange
49 50	,,	,,	,,
49 50	,,	,,	
50 5	,,	N.	
50 50	,,	S.	
53 43	N. of ♂	Zenith	Head, orange; train, blue
54 10	Leo	Horizon	
55 12	,,	S.	
55 20	,,	,,	
56 10	,,	Horizon	"Pear" shaped.
57 45	Below Regulus	S. W.	
57 52	,,	,,	
58 28	Under ♂	N.	
58 40	Leo	S.	Small
14 59 5	,,	,,	
15 4 7	ζ Leonis	Horizon	"Pear" shaped.
4 27	,,	,,	

maximum



Cape Mean Time.	Direction. From	To	Remarks.
h m s			
15 4 35	♂	N.	
6 5	Leo	Horizon	
7 40	,,	Zenith	
10 45	,,	Horizontal to S.	Very bright.
11 50	♂	N.	
12 5	♀	S.	
13 43	♀	Regulus	
13 50	Regulus	S. W.	
14 30	,,	S.	Long train.
15 15	,,	Zenith	,, orange.
15 50	Below ♂	Horizon	Large.
16 50	,,	S.	
19 30	,,	Horizon	
20 0	,,	N.	
21 35	,,	S.	Orange.
25 8	,,	N	
25 11	,,	,,	
27 1	ι Leonis	♂	
27 10	ϑ Leonis	Horizon	
27 55	,,	N.	
28 0	,,	S.	
28 50	ζ Leonis	N.	
29 40	β Leonis	♀	
29 50	ϑ Leonis	S.	
30 4	,,	W.	Red train.
30 10	α Hydræ	Horizon	
30 55	β Leonis	N. horizon	
34 10	β Corvi	Horizon	
34 30	ϑ Leonis	,,	
36 10	η Leonis	β Corvi	Orange.
36 35	ϑ Leonis	Horizon	
36 35	♂	W.	
37 10	Regulus	S. W.	Orange, large.
37 25	,,	α Centauri	Large, long train.
39 55	γ Leonis	W.	Orange and red, bright.
41 10	♂	Horizon	
41 30	η Leonis	β Corvi	Small.
41 50	ζ Leonis	S. W.	
41 55	Regulus	,,	
42 15	ζ Leonis	Horizon	
42 40	,,	S. W.	Orange blue.
43 5	β Leonis	S.	
15 43 25	Leo	E. Horizon	Orange, train green.

Cape Mean Time. h m s	Direction. From	To	Remarks.
15 43 30	Regulus	α Centauri	,,
43 50	,,	,,	,,
43 52	,,	,,	,,
46 15	δ Leonis	β Corvi	
46 17	,,	,,	
46 50	,,	,,	
46 55	,,	,,	
47 0	Regulus	α Centauri	Orange, large.
49 30	ζ Leonis	Horizon	Orange and green.
49 40	,,	,,	,,
50 30	δ Leonis	,,	Small.
51 10	,,	,,	Green, small.
51 15	,,	S.	,,
52 10	,,	β Corvi	Orange and Green.
53 50	ζ Leonis	Horizon	
55 25	,,	W.	,,
57 15	Regulus	S.W.	Small.
58 7	ε Leonis	,,	,,
15 59 0	ζ Leonis	Horizon	,,
16 4 0	Regulus	S.W.	
7 40	δ Leonis	Horizon	Orange.
12 0	,,	Spica	Orange and Green.
26 40	Regulus	β Corvi	
16 27 20	δ Leonis	Spica	

(Total 134.)

Royal Observatory,
Cape of Good Hope, Nov. 17th, 1868.

On some points connected with the Rediscussion of the Observations of the Transit of Venus, 1769. By E. J. Stone, Esq.

At the Séance de l'Académie des Sciences, 1869, January 4, M. Faye brought forward a defence of M. Powalky's discussion of the observations of the transit of *Venus*, 1769. The weight which, irrespective of its merits, is necessarily attached to a paper read before so learned a body and by so distinguished an Astronomer as M. Faye, compels me at once to express the grounds on which I am unable to accept either the methods or the results of M. Powalky's discussion.

My first inquiry shall be respecting the foundation of M. Faye's remark, that M. Powalky "a eu le mérite d'élucider le premier la cause d'une erreur compromettante pour la science."

I will lay down at once a few propositions to which I shall have frequent necessity of appealing.

1. There are two phenomena at internal contact which were

observed in 1769. The so-called real internal contact, and the apparent internal contact. These phenomena were in the transit of 1769 separated *roughly* by about 16ˢ.

2. An observation of an apparent contact is not an observation of a real contact, and *vice versâ*.

3. In any *legitimately* conducted investigation, the use of an apparent contact for a real contact, or *vice versâ*, must lead to an erroneous value of solar parallax to the extent which an arbitrary change of about 16ˢ in the incongruous observation would affect the final result.

4. If observations have to be rejected because of their incompatibility with the general run of the discussion, then every such rejection enormously increases the presumption against the truth of the investigation which requires such rejection.

5. The absolute necessity of rejecting *one* good observation would, in itself, be conclusive evidence against the truth of the deduced value of solar parallax or the logic of the method applied.

M. Powalky attributes the difference between his value and that deduced by Encke to his employment of more accurate longitudes than those available to Encke. The reason why I am compelled to dissent from this assertion will best appear by a consideration of the observations made use of by M. Powalky at those stations where durations were observed. We shall thus clear ourselves entirely from the effects of these more accurate determinations of longitude.

Wardhus Station.

M. Powalky employs, for internal contacts, Sajnovics' observation at ingress, "Certissimus ut ajebat." At egress, Hell's observation, "videtur aliqua gutta nigra intra limbum Solis et Veneris ante contactum fieri."

We have here one violation of (3).

Kola.

The observations at this station are not employed by M. Powalky.

Hudson's Bay.

We have here two observed durations from real contact to real contact. M. Powalky employs both observations at ingress, which are well represented in his residuals. He rejects, on account of discordance, both observations at egress. The observed durations agree within 1ˢ. We have here two violations of (4) if not even of (5). M. Powalky's solution would make each of the observers to have caught the real contact at egress *about* 13ˢ too soon.

St. Joseph.

M. Powalky has used the three observations of Chappe, V. Doz, and Medina, as observations of real contact. He has, however,

only given to the three observations the weight of one. The residual of the mean of the three observations at ingress amounts to $-7^s{\cdot}5$.

Otaheite.

M. Powalky follows Encke in considering the observations of apparent contact of the "penumbra as dark or nearly as dark as the planet itself," as observations of real contacts.

He employs, at ingress, the observations of Green and Solander. He rejects Cook's observation, with a residual of $-15^s{\cdot}6$. At egress, M. Powalky uses Green's observation with a weight only of $\frac{1}{2}$: the residual is $-7^s{\cdot}8$. He rejects Cook's observation, with a residual of $-17^s{\cdot}8$.

Now, upon this I would remark that, if the observations are observations of real contact, M. Powalky has here, by the rejection of Cook's observations and the little weight attributed to Green's observation at egress, violated (4), if not (5), several times. If these observations are not observations of real contacts, M. Powalky has violated (3) by using Green's observations. In these durations, be it remembered, we are free from the effects of longitude.

Of the ten complete durations from internal contacts (all that are available) which I have used in my paper and which are there, at least, perfectly represented, M. Powalky has employed, in their entirety, only four. To three of these combined he gives the weight of one only. The fourth duration, that at Otaheite, he partially rejects by giving only a weight of one-half to Green's observation at egress.

To these he has added a duration obtained from an ingress observation of Sajnovics' and an egress observation of Hell. This is the only duration well represented in his solution.

With respect to the observations at stations where durations were not observed I shall have but little to remark.

The selection of Hitchins' observation at $7^h\ 28^m\ 57^s{\cdot}6$ A.T. as the representative observation of real contact, in opposition to the whole body of observers in England and Southern Europe, appears to me unsound. If we know better than these observations can teach us, we need not appeal to them, but if we do appeal to them for information we must accept fairly the information they afford. The greater the discordances amongst the observations—the greater the difficulties under which the observations were made—the more unsafe must become any selection of an extreme value, like that of Hitchins', to the exclusion of the mean.

The selection of Shippen's observation as the representative observation of real contact for observations made with factor about 27, on the American continent, appears to me also objectionable. There are at least seven other observations on this station, all of which appear to give a later time than Shippen's for the disappearance of the connecting ligament at ingress.

An alteration in the value of solar parallax of 0″·4 is a question only of less than 2ˢ between Wardhus and Greenwich, and of less than 7ˢ between Wardhus and Shippen's station, Philadelphia. The observations of Hitchins and Shippen, selected from groups, and each early and extreme observations of real contact, are given full weight. I think it will hardly escape notice how M. Powalky's solution fails, where the real strain falls upon it, to reconcile the St. Joseph, and more particularly the Otaheite observations with those made in the extreme northern stations. I must remark, M. Powalky has considered the question of adopted semi-diameter of so little importance in the discussion, that he has adopted Encke's value deduced from the employment of very different data.

We knew, before M. Powalky's paper appeared, that by adopting different data different results could be obtained. What we did not know was any cause for these discrepancies. The selection of the material had been based upon no intelligible principle, and no one had been able to reconcile the whole of the durations. I now leave M. Faye's defence of M. Powalky's work to the judgment of competent astronomers. I, at least, do not consider that "il a eu le mérite d'élucider le premier la cause d'une erreur compromettante pour la science."

I now pass to my own work upon this important question. In my opinion all the difficulties which have been experienced in the discussion of the observations of the Transit of *Venus* in 1769, are to be traced to a single source—mistaken notions with respect to the adoption of semi-diameter, and insufficient recognition of the nature of the uncertainties thus introduced. My view will perhaps be more clearly understood as follows. If we knew the true value of the solar parallax, the true R.A. and N.P.D. of the Sun and of the planet, the true semi-diameter of the Sun and planet, we could compute, with the utmost accuracy, the time of *real* contact for each station. If we knew the apparent semi-diameters of the Sun and of *Venus*, when projected upon the Sun's disc, and enough of the theory of irradiation, we could compute with the same accuracy the apparent internal contact; in fact, every phase of the phenomena presented from the real contact to the exterior contact. Now, the so-called observed real internal contacts which are the times of first appearance or disappearance of the connecting ligament (*i.e.* the times at which the observer first recognises the existence of the ligament) can no more, at present, be directly computed than the so-called apparent contacts. The two phenomena stand in much the same position. They are both complicated by the effects of irradiation. If we make use of durations, every observed duration, either from apparent contacts or real contacts, will lead to an equation of condition of the form $\frac{x_1+y_1}{2}+a_1\,d\pi =$ observed discordance; a_1 is a constant depending on the observer's position, $d\pi$ the correction to the assumed parallax, x_1 and y_1, two functions of errors of relative

tabular R.A. and N.P.D., and of *correction for semi-diameters, corresponding to the observed phase at ingress and egress respectively.*

Now the observer really observed *some phase.* If we knew the true values of x_1 and y_1, each equation would give the true value of $d\pi$. Unfortunately we know nothing about x_1 and y_1. Their values must be deduced from the observations themselves as approximately as we can find them. If we can find several observations which appear to be observations of the same phase, we can assume the corresponding x and y, one or both, to be the same for these several observations. The deviation of the phase really caught by the observer from our mean phase is now really the so-called error of observation, which is, in fact, nothing more than the error committed in assuming that the phase observed is the mean phase.

The attention of observers having been chiefly directed to observations of apparent contact and the so-called real contacts, the observations naturally group themselves around two mean phases near the time of real contact and the time of apparent contact respectively. The observations of these two phases differ by about 16ˢ. If we had enough observations, we might assume all these contacts, real and apparent, to be observations of the same phenomenon. The probable error of an observation measured by the residual errors would in this case be very large, for we include in these residuals all the errors made by assuming that the observations refer to the mean phase. A very large number of observations well distributed would thus be required in order that a result of any accuracy should be deduced. We clearly have not enough observations for such rough work.

If we advance one step beyond this, and assume that all the observations of real contact correspond to one mean phase, all the observations of apparent contact to another mean phase, we shall have made a considerable reduction in the probable error of each observation. We shall thus require, comparatively speaking, a small number of observations, even although we have thus introduced one more unknown quantity. This, of course, depends upon the attention of the observers having been directed to two essential points; the observer's attention having been directed to the real and apparent contacts, the observations naturally group themselves around two corresponding mean phases. I have, therefore, adopted two unknown quantities x and y; one corresponding to an apparent, the other to a real mean phase. And here a word or two on M. Faye's assumption, with respect to the accuracy which is thus to be obtained. That accuracy depends only, upon the factors of parallax correction involved in the equations of condition and the closeness with which the observations of apparent and real contacts group themselves around their respective mean phases. The accuracy really obtained can be judged of solely by the smallness of the final residuals. It may, perhaps, be said, that in thus

introducing one more unknown quantity, the final residuals must necessarily be smaller than those otherwise obtained. To this I reply that the new unknown quantity which I have introduced amounts to very little more than the arbitrary selection of observations which were assumed to be real internal contacts at those stations where both apparent and real contacts were observed, a thing which has always been previously done. At all events, I use both apparent and real contacts, and I have introduced no more unknown quantities than are absolutely required.

There is one important point that must here be borne in mind. The x and y introduced in my equations of condition are determined by making the residual errors the least possible. This is the analytical consideration from which they are determined. But x and y have been introduced to correspond to the observed mean phases of apparent and real contact. They have, therefore, a physical interpretation. We must, therefore, have, if our solution is a true solution, $\frac{x-y}{2}$ equal to the differences in time between these two mean phases. Now, amongst the observations at the five stations at which durations have been observed, some three differences between the two phases may be collected. Their mean value agrees so closely with that deduced from my equations that all these mixed observations might have been included without changing my result. I look upon this point as one of the utmost importance. Had the solution been a forced solution the probability of such an agreement would be small indeed.

The ten durations are all satisfied. The necessary condition is satisfied by the value of $\frac{x-y}{2}$. What reasons, therefore, have we to doubt the result? It must be remembered by M. Faye that the onus *now rests upon him* of proving, and not asserting, that some one of my observations has been erroneously referred to mean phase in such a way as to seriously affect my results; or that there are observed durations which are without doubt correct, and which are inconsistent with my solution.

Now to what extent has M. Faye been enabled even to throw doubt upon my distribution of observations? With respect to the Wardhus observations, he *thinks* that "fulmen $32^m\ 48^s$" is an observation of real contact at ingress, and he would prefer using this and Hell's observations of real contact at egress to using apparent contacts. Now the question is, are we certain that fulmen $32^m\ 48^s$ represents any attempted observation of real contact? I doubt it for the following reasons:—Professor Littrow states that "fulmen $32^m\ 48^s$" only occurs in the darker ink in which some corrections and explanatory words were afterwards added. In the fac-simile of this important part of Hell's Journal, which Professor Littrow has done me the honour to forward to me, I

find this observation completely barred off from Hell's observations; in fact, cramped in at the top of Sajnovics' observations.

I remark also that the words employed in describing the phenomena at ingress are similar to those employed in describing the corresponding phenomena at the egress. We have a very clear description of the phenomena observed at the real contact at egress. The description is certainly very different indeed from the laconic use of a single word, *fulmen*. I believe that this most probably refers to that apparent flowing in of the light upon both sides which is so clearly described by Wales at the Hudson's Bay observations of ingress. "We took for the instant of the first internal contact the time when the least visible thread of light appeared behind the subsequent limb of *Venus*, but before that time *Venus's* limb *seemed within that of the Sun*."

I do not say that this is the true explanation of the introduction of these words, but I do say that "fulmen $32^m\ 48^s$" is not entered as an observation like the others in due order. It is completely barred off from Hell's observation, and its meaning is exceedingly doubtful. Having at this station two complete observations of apparent contacts, I rest my discussion on this sure basis in preference to the introduction of materials so doubtful as the time opposite this entry of *fulmen*.

It may perhaps be thought that this is a critical point in my argument. Such, however, is not the case. If I had used the "fulmen $32^m\ 48^s$," as an observation of real contact, my results would have been but little modified. Take it as an observation of real contact, which I do not however believe it to be, and combine with Hell's observation of real contact at egress, and we have a duration which is represented with a residual error of about 9^s *by my solution:* a discordance not large enough to alter much the resulting parallax, but large enough to justify the refusal to introduce so doubtful an observation. I preferred, and I still prefer, to use the more certain materials of the apparent contacts. With respect to the accuracy with which these apparent contacts can be observed, the discordance of Hell's and Sajnovics' observations is only $4^s{\cdot}5$.

With respect to the St. Joseph observations, I have so fully discussed this point in my answer to Mr. Newcomb, that I shall say but little here. I believe Chappe's words; and Pauly's observation of some contact at egress, 22^s before Chappe's observation, *proves* that Chappe did not observe a real contact or the *first* appearance of the connecting ligament. I believe it proves *à priori* that he really caught some phase much nearer the mean phase of apparent contacts. If however there is any doubt upon the point, appeal must be made to the residuals. Now with my interpretation all these residuals, and all other residuals, are in beautiful accord. This is admitted. On M. Faye's assumption all the residuals are in discord. I am really quite unable to understand M. Faye's inference from the fact that, if we arbitrarily change the interpretation of the egress observa-

tions at St. Joseph from apparent to real contacts, we throw the residuals which were all before in agreement into disagreement. M. Faye infers that this proves that the agreement of all the residuals was before forced. I take it most clearly to prove that M. Faye's interpretation is not the true one. If we knew the true values of the phase correction at ingress and egress there would remain in the residuals nothing but the error of assumed parallax, which, as my result differs only by $0''.05$ from that which M. Faye wishes to be considered the truth, would amount to less than 2^s. Moreover, with M. Faye's interpretation, we have all the other residuals thrown into confusion, and particularly the Hudson's Bay observations, which are undoubtedly observations of real contacts, each saddled with a discordance of the same sign, and amounting to $7^s.9$ and $8^s.9$ respectively. We have, moreover, with M. Faye's interpretation, two values of solar parallax $8''.91$, identical with my own, from the apparent contacts, and $8''.70$, from M. Faye's attempt to improve the real contact observations. I must here, as before, recall to M. Faye's mind that, *cæteris paribus*, harmony in the results is a proof of truth. If he denies the truth, he must prove, not assume, that I have referred my contacts to a wrong mean phase, or that there are well-observed durations incompatible with my solution.

I next pass to the Otaheite observations. I have taken the apparent contacts of the "penumbra as dark, or nearly as dark, as that of the planet itself," to be apparent contacts. M. Faye admits that they are not real contacts. He thinks however, that they are some phase between real and apparent contacts. Such an impression can only be tested by the agreement or disagreement of the results. Such a test is dead against M. Faye's impression. The observations are all in agreement. But what reasons does M. Faye bring forward that render such an impression even plausible? He thinks that the form of the reflectors made use of by all the observers must have been distorted, so that the image of a point was no longer a point. The observers made no mention of any want of definition. M. Faye then assumes that such want of figure, which would undoubtedly injure definition, would seriously interfere with an observation of apparent contact when the limb of the planet and of the Sun are similarly reflected. I have not the slightest belief in the production of such an effect, to any important amount, even if the cause did exist. M. Faye must remember that I have allowed 3^s for probable error of each contact. We are not, fortunately, in this inquiry concerned with $0^s.5$ of a second. My own explanation of this penumbra is that it is nothing more than the fuzzy light which appears around the edge of a dark body projected on a bright body like the Sun. I saw a similar phenomenon at the last transit of *Mercury*. The observers in 1769 were impressed with the opportunity presented by a transit of *Venus* to detect the presence of an atmosphere; any such appearance of

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not be inconsistent with their evidence. This point had re-
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I considered the evidence thus afforded was confirmatory
general solution. I would remark that the observations
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any information on the point, considerable magnifying powers,
what is of still more importance, powers very nearly equal.
may reasonably suppose that their instrumental equipments
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the best available at the time. From their drawings and
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The observations made in England and Southern Europe were
made under most unfavourable circumstances. The Sun at
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stray light about the edges of *Venus* would, if it once attracted attention, be naturally deemed of some importance. The name penumbra is by no means an unsuggestive name for such an appearance to any one who did not attribute the appearance to its true cause, irradiation.

Such are the points in which M. Faye takes exception to my work.

He thinks that I ought to have used something, we know not what, as an observation of real contact at Wardhus. Had I used this it would have but little modified my result. It may be an observation of real contact: it may be, as I have suggested, an indication of the appearance of light nearly all round the planet. I only say that it is not inconsistent with my solution in any case, but that I prefer to rest the solution upon a more intelligible basis. M. Faye prefers to use Chappe's observation at egress as a real contact. I say that Pauly's observation shows this to be inadmissible, and the residuals say the same. Chappe's own words, in my opinion, tend to the same conclusion. M. Faye thinks that the Otaheite observations of apparent contacts of penumbra are something between a real and an apparent mean phase. The residuals say what the observer's own words appear to indicate, that they are observations of apparent contact. Assume that, in all the cases specified, M. Faye's interpretations were as simple and as reasonable as my own (I am very far, indeed, however, from granting this) — what then? my assumption leads to perfect harmony amongst the observations themselves. M. Faye throws all again into chaos. M. Faye has certainly overlooked the fact that the observers did observe something. The so-called errors of observation would be reduced to nothing if we only knew the proper corrections to reduce all the observations to the same phase. With respect to the probable error of my result, I have assumed the probable error of the deviation of such observations from the corresponding mean phase to be 3^s. The probable error of the resulting parallax is then less than $0''.03$. All I can say is, that the residuals do not appear to require a larger assumption of probable error. However, if we assume 6^s, the probable error of the result would be only be about $0''.05$. The very form which all discussions are now taking upon transits of *Venus* and transits of *Mercury;* questions of irradiation; questions whether observations refer to real or apparent contacts; separation, justly enough, of observations of contacts of *Mercury* into classes; shows the flood of light which has been let into this question since the appearance of my paper. Transits of *Mercury* have taken place before: the phenomenon of the black drop was known; but it was not known that the value of the solar parallax which resulted from the discussion of the observations of the transit of 1769 was entirely a question of the reference of a few observations to apparent or real contacts. It was not known that by interpretations put upon the observer's words, at least as simple as any interpretations which can be put upon them, the whole of the observations collected could

be reduced into perfect harmony. And that the value of the solar parallax which will *alone* reduce these observations to harmony is a value so close to what we now believe the truth, that no one method, and probably not the mean of all the other methods, can say how much that error may be. I leave my work with confidence to the judgment of astronomers, knowing that justice will be fully done, and whatever my real claims may be they will ultimately be fairly recognised.

It may perhaps be desirable that attention should be called to the agreement which holds between the results of my paper "On the Rediscussion of the Transit of Venus, 1769," and the numerous observations of ingress obtained in England, Southern Europe, and on the American Continent; also with a few scattered observations of egress. These observations were not employed in my discussion. Their introduction would not, in my opinion, have added sensibly, if at all, to the true weight of the final result. It is however of course necessary that my solution, if a true solution, should not be inconsistent with their evidence. This point had received my most serious consideration before the appearance of my paper. I considered the evidence thus afforded was confirmatory of my general solution. I would remark that the observations which I have made use of are the best for several reasons; they are free from questions of absolute time, a point of paramount importance. The observers at these stations all used, so far as we have any information on the point, considerable magnifying powers, and, what is of still more importance, powers very nearly equal. From the importance of the observations at these selected stations we may reasonably suppose that their instrumental equipments were the best available at the time. From their drawings and verbal descriptions their attention appears to have been drawn to the observations of the same two principal points. These observations were made under fair atmospherical conditions. We cannot, therefore, expect to have so good an agreement amongst miscellaneous results, collected under unfavourable circumstances, or with very different powers and complicated with errors of absolute time, as amongst the results made under especially favourable conditions and entirely free from all systematic errors except the rates of their clocks.

The observations made in England and Southern Europe were all made under most unfavourable circumstances. The Sun at Greenwich was less than 5° high at internal contact, and lower considerably in France, most of the observations having been made at a less altitude than $3\frac{1}{2}$°. Many of the observations were made with low powers; the results are all complicated by errors of absolute time. The attention of the observers appears to have been directed to very different methods of estimating the time of real contact; some even waited for the time when the disk of *Venus* appeared again perfectly round,—a very rough observation, I presume, at such low altitudes. Under such circumstances

great discordances might be expected. The observations are, in fact, so discordant that only one method appears to me available for making any use of them. If we collect all the observations for which the factor of parallax is nearly the same, and which appear or profess to be observations of apparent contact, and take their mean; and if we proceed exactly in the same manner with the observations of real contact, the mean of the two results thus obtained should correspond with some accuracy to a phase half-way between the apparent and mean phases. This method may appear rough, but the materials are rough, and no objection can be urged against its fairness. From 89 observations thus available I have found a residual of + 22ˢ. My method may perhaps be better understood if we confine our attention to the observations made at the Royal Observatory, Greenwich. We have here four observations of "Regular circumference of Sun and *Venus* in contact." The mean apparent time is $7^h\ 28^m\ 45^s$. We have five observations of "Completion of thread of light." The mean is $7^h\ 29^m\ 23^s$ A.T. The mean of these times, $7^h\ 29^m\ 4^s$ A.T., should correspond to the mean phase very nearly. The residual for this time for this station is + 25ˢ, agreeing pretty closely with the general mean. I will now deduce from my general solution what should be the residual for mean phase at Greenwich, assuming the mean of Hell's and Sajnovics' observations of apparent contact at Wardhus to be free from error. We have from these observations,

$$35^s + 10{\cdot}24\,d\alpha - 16{\cdot}24\,d\Delta - 44{\cdot}5\,d\Pi - 19{\cdot}7\,\varepsilon = 0.$$

If A and T are the residuals for apparent contact and true contact at Greenwich,

$$A + 10{\cdot}14\,d\alpha - 16{\cdot}36\,d\Delta - 48{\cdot}4\,d\Pi - 19{\cdot}7\,\varepsilon\,d\Pi = 0.$$

or sensibly, since $d\Pi = 0''{\cdot}42$,

$$A = 35 + 3{\cdot}9\,d\Pi = 36{\cdot}6$$
$$T = A - 16{\cdot}6 \quad = 20{\cdot}0.$$

Consequently

$$\frac{A+T}{2} = 28^s{\cdot}3.$$

The quantity obtained from the Greenwich Observations is 25ˢ.

I think this agreement is sufficient to show that the Wardhus Observations and the Greenwich Observations are reconciled by my solution. The residual for Greenwich, 25ˢ, is rather larger than the general mean, but this general mean is drawn down by some observations made in the South of France, where the circumstances of observation were still much more unfavourable than at Greenwich, as stated above.

I shall next pass to the consideration of the numerous observations made on the North American Continent. These observations were made under fair atmospherical circumstances, and



their agreement is good, especially when free from uncertainties of absolute time. The instrumental equipments of some of the observers were, however, rather imperfect, and uncertainties still hang over the determinations of the longitudes of some of the stations, and even of the exact position of some of the observing stations. Here, again, I shall lay more stress in representing the mean of the observations, than in representing with great accuracy one selected observation.

The following residuals are extracted from Encke's discussion, with corrections to assumed longitudes extracted from a table in M. Powalky's discussion.

Station.	Observer.	s	
Philadelphia	Shippen	+21	real contact.
,,	Williamson	30	apparent contact.
,,	Thomson	27	
,,	Prior	29	
,,	Erving	31	

Shippen states that he tried to observe the completion of the thread of light. I shall take, therefore, Shippen's to be an attempt at a real contact, and the other observations as attempts at apparent contacts.

Station.	Observer.	s	
Norriton	Lukens	40·9	apparent contacts.
,,	Smith	37·9	

The description of the phenomena observed is not very clear, but the observations appear to me to be attempts at apparent contacts; Ritterhouse observed, at the same station, some connexion long after the times given for these contacts, which can therefore, only be assumed to be attempts at apparent contacts.

Cambridge.

Professor Winthrop, + 9ˢ·2. Real contact.

This observation is of great value. The observer's position is well known. He took great care to determine the time with accuracy.

Station.	Observer.	s	
Providence	West	+1·0	real contact?
Wilmington	Poole	+3·9	real contact.
Lewes	Biddle	−9·0	real contacts.
,,	Bailey	−5·0	
Newbury	Williams	−0·3	real contact.
Baskeridge	Stirling	−0·0	real contact.

These results would give the following residuals,—

For apparent contacts	+32·6	mean 17ˢ·6.
real contacts	+ 2·6	

The residual for the real contact is made small by the observations at Lewes, and is, of course, affected by any error in the assumed longitude ; no observations are given to test the accuracy with which the time was determined.

Then, assuming as before the Wardhus observation to be correct, I deduce from my solution for the mean station of these observations

For apparent contacts	+28·1	mean 19ˢ·8.
real contacts	+11·5	

The mean results of calculation, based upon the Wardhus observations and my solution, and the results of the observations of these fourteen observers, agree within about 2ˢ. The residual for real contact from my solution is almost identical with the means of Shippen's, Winthrop's, and Poole's results.

If the means of these American observations are taken without any attempt to discriminate between the phases, the residuals cannot be expected to agree so closely. The difference is, however, only about 5ˢ. There is a most valuable group of observations made at St. Domingue by Pingré, Fleurieu, La Filière, and Destourès. The observations are entered as

" *Contact intérieur des bords du Soleil et de Vénus.*

Temps vrai. h ′ ″	
2 44 45	Selon M. de Fleurieu.
2 44 41	,, M. le Chevalier de la Filière.
2 44 50	,, M. Destourès.
2 44 44	,, M. Pingré."

I assume these observations to be those of apparent contact or of contact of limb with limb. Assuming, as before, the Wardhus observations as correct, I find that the mean of these results is represented within 1ˢ by my general solution. If we assume the mean of the Otaheite observations to be true, we can represent these observations within 3ˢ.

With respect to the egress observations, if we take the mean of Green's and Cook's observations to be the true time of apparent contact at Otaheite, and assume the results of my re-discussion, we can represent the observations with factor about + 42·6 *d* Π viz. the real contact observations of Lowitz's, Inochodsow, ar Kraft's at Gurief and Orenbourg within 5ˢ; the mean of Mo and Dollières' observations at Batavia and Pekin, with fa about + 28·4 *d* Π within about 4ˢ; the apparent contact Collas, at Pekin, within about 2ˢ. These are really all checks which we can apply to the Wardhus and Otal

observations respectively which are not applied in my re-discussion, and these checks appear to me all confirmatory of the interpretations I have put upon the Wardhus and Otaheite observations.

The only point which does not appear to me perfectly confirmatory of my solution is the apparently rather close agreement of the Hudson Bay observations of ingress, which are undoubtedly real contact observations, with the apparent contacts at Wardhus. Upon this I can only remark that the longitude of this station depends upon the discussion of a few occultations made nearly one hundred years ago; that this difficulty arises entirely from the great difference in the longitude deduced from these same occultations by M. Powalky from that previously deduced by Encke; that M. Powalky's longitude makes the egress observations entirely disagree with other undoubted observations of real contact; *that the durations at this station are perfectly represented in my solution.* The uncertainties still resting on the longitudes of this station and of St. Joseph afford the best justification of the method I adopted in making the discussion depend solely upon durations. I am more than ever convinced of the extreme importance of observing the so-called apparent contacts as well as the so-called real contacts. It will be seen from what precedes that a mean between apparent and real contacts is a phase represented with accuracy. In conclusion, I may, perhaps, be allowed to suggest that discussions which reject the observations at *Otaheite or Wardhus and Kola* as inconsistent with their results are not of very much weight. The strain on the truth of the result is in these cases really slipped, not borne.

My object in this addition is simply to afford independent proofs that the adopted times for Wardhus and Otaheite are not much in error; if these observations are sensibly correct, there can be no doubt about the value of solar parallax.

Addition to Mr. Proctor's Paper on the Transit of Venus in 1874. (*Monthly Notices for March 12th*, 1869.)

(*Communicated by Mr. Proctor.*)

The four following corrections refer to printer's errors:—

Page 213, in the expression for V, $d^{\frac{1}{2}}$ *should be* $d^{\frac{1}{4}}$.

Page 215, line 14 from bottom, *for* elliptical *read* ecliptical.

Page 217, line 9 from top, *for* l' *read* λ'.

Page 217, line 8 from bottom, *for* 54° 27′ *read* 44° 27′.

The last correction is important, though the context (*see* p. 218, line 12) sufficiently shows what was intended.

I have gone very carefully through the calculations on which the results in my paper were founded, and have detected a slight error affecting the last three longitudes in the table on p. 217. The error (one of transcription) does not invalidate, but on the contrary rather strengthens, the conclusions arrived at in my paper. As the first of the three longitudes just named was also wrongly printed, it seems best to give the whole table, as corrected :—

	Lat.	Long.
(i.) The place at which first internal contact is most accelerated lies in	° ′ 39 45 N.	° ′ 143 23 W.
(ii.) The place at which first internal contact is most retarded lies in	44 27 S.	26 27 E.
(iii.) The place at which last internal contact is most accelerated lies in	64 47 S.	114 37 W.
(iv.) The place at which last internal contact is most retarded lies in	62 5 N.	48 22 E.

I have calculated the positions of the corresponding places with the elements given by Mr. Airy in the *Monthly Notices* for December 11th, 1868, p. 34. These elements are not strictly congruous, it will be noticed, but I use them without change; except that I have ventured to alter the value of the Sun's declination, which is obviously incorrect. The places corresponding to (i.) and (ii.) lie respectively in north and south latitude 37° 13′ and in longitude 138° 38′ W. and 41° 22′ E., respectively. The places corresponding to (iii.) and (iv.) lie respectively in north and south latitude 60° 1′, and in longitude 136° 11′ W. and 43° 49′ E., respectively. The distances of these four places from those tabulated above I find to be (by the method of mid-latitudes) 314·0, 920·2, 764·5, and 208·7 miles, respectively.

On some Effects of the Comparative Clinging of the Limb of Venus to that of the Sun in the Transit of 1874, *as compared with that of* 1882. By E. J. Stone, Esq.

There is one point of great practical importance which must not be overlooked in estimating the relative values of the Transi of *Venus*, 1874 and 1882, and in determining which of the thods proposed for utilizing the Transit of 1874 is the more able. The chord described in 1874 is considerably less th corresponding chord in 1882. M. Puiseux has shown, ho that very considerable differences of duration can be obtai the Transit of 1874, and his results have been confirmed Proctor. The same cause, however, which renders this renders the observations themselves less accurate. The this short Note is to obtain an estimation of the injurio

upon the observations themselves arising from the slow motion of *Venus* across the Sun's limbs, and to trace the consequences upon the methods proposed by the Astronomer Royal and M. Puiseux for utilizing the Transit of 1874. The difficulty in deducing a value of Solar Parallax from observations of contacts of *Venus* and the Sun is that of referring the observations to the same phase. We are really, therefore, concerned with estimations of angular measures by their equivalent times. I find from Mr. Hind's calculation, *Comptes Rendus*, 1861, July 22, that, with the same assumed semidiameter of *Venus*, the time from external contact to internal contact in the Transit of 1874 is no less than 29^m, whilst in that of 1882 it is only $20^m\ 19^s$. From this I conclude that, if e be the probable error in time of an observation in 1882, the corresponding probable error for the Transit of 1874 will be

$$e \times \frac{29^m}{20^m\ 19^s} = e\,\frac{1740}{1219}.$$

A difference of durations of $20^m\ 19^s$ is, therefore, in 1882 as valuable as one of 29^m in 1874. Other differences are effective in like proportion. It appears to me impossible to secure good observations in 1874 with such differences as those mentioned by Mr. Proctor. It must be remembered that it is useless to discuss differences obtained from the consideration of stations at which the necessary observations cannot be made on account of the small altitudes of the Sun at the times of internal contacts. For my part I should at once exclude from consideration all stations with altitudes of the Sun, at either ingress or egress, much less than 10°. I consider even this too low, unless a good deal has to be sacrificed in parallax-factor to secure greater elevation.

How small the value of observations made within 5° of the horizon is, will at once appear by a comparison of the English and French observations of the Transit of 1769 with other observations made at greater altitudes of the Sun.

The relative effects of the increased probable error of internal contacts in 1874, on the method proposed by the Astronomer Royal and M. Puiseux, is seen from the following formulæ:—

Let l be the probable error of a longitude determination,

e that of an internal contact,

then (constant)^2 (probable error of parallax)^2

$= 4e^2$. In the method of durations.

$= 2(e^2 + l^2)$. In Mr. Airy's method.

the relative weights are therefore

$$\frac{\text{Puiseux}}{\text{Airy}} = \frac{(\text{Puiseux constant})^2}{(\text{Airy constant})^2} \cdot \frac{1}{2}\left\{1 + \frac{l^2}{e^2}\right\}$$

Since, therefore, l is constant for all times, and e is greatly

increased in 1874, we see at once from the formula that the method of durations contrasts much more unfavourably with that of absolute time determination in 1874, than in that of 1882 or in that of 1769. This is probably the ground upon which the Astronomer Royal still gives his preference to absolute time determinations for the Transit of 1874. The formula can very approximately be turned into numbers. The Astronomer Royal considers that we are now in a position to determine longitudes with a probable error of 1^s. In my re-discussion of the Transit of *Venus*, 1769, I have assumed 3^s for the probable error of an internal contact. This appears to cover the residuals, but it has been considered too small by some astronomers. Taking it, however, at 3^s for 1882, we must assume $e = 4^s{\cdot}28$ for 1874. In order then that the probable errors of M. Puiseux's method and that of Mr. Airy should be equal, we must have differences of duration of 31^m in M. Puiseux's method to correspond to differences of 22^m in Mr. Airy's. This of course depends upon the possibility of determining longitudes within 1^s. I do not think that the probable error of internal contacts has been over-estimated.

The method proposed by the Astronomer Royal possesses also another advantage; it removes the necessity of attempting to push observers into the Antarctic regions. Kerguelen's Islands and the Macdonald Islands are, however, favourable positions for both methods, and there appears every reason to avail ourselves of both methods to at least this extent. From the slow motion of *Venus* from the Sun's limb, I do not consider the Transit of 1874 a favourable one.

Blackheath,
1869, *April* 8.

Occultations of Stars by the Moon and Phenomena of Jupiter's Satellites, observed at the Royal Observatory, Greenwich, from 1868, *January, to* 1869, *March.* Communicated by G. B. Airy, Esq., Astronomer Royal.

Occultation of Stars by the Moon.

Day of Obs. 1868.		Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
Feb. 28	(*a*)	μ Ceti, disappearance	Dark	9 24 45·7	J.C.
		μ Ceti, reappearance	Bright	9 47 51·1	J.C.
Mar. 1		θ^2 Tauri, disappearance	Dark	6 38 24·5	E.
		θ^1 Tauri, disappearance	Dark	6 38 37·0	E.
May 4	(*b*)	l^2 Virginis, disappearance	Dark	9 7 35·6	C.
	(*c*)	l^2 Virginis, reappearance	Bright	10 21 36·3	C.

Day of Obs. 1868.		Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
May 27		18 Leonis, disappearance	Dark	10 30 59·3	D.
Dec. 27	(*d*)	B.A.C. 1526, disappearance	Dark	6 27 51·2	J.C.
1869.					
Jan. 24		119 Tauri, disappearance	Dark	8 14 33·3	D.
		120 Tauri, disappearance	Dark	8 47 42·5	D.

(*a*) Cloudy.
(*b*) The disappearance did not seem to take place so suddenly as usual.
(*c*) Thought to be a little late.
(*d*) Uncertain to a second; clock beats scarcely audible from high wind.

Phenomena of Jupiter's Satellites.

Day of Obs. 1868.	Satellites.		Phenomenon.	Mean Solar Time. h m s	Observer.
Jan. 15	III.		Occult. reappearance, first contact	6 4 15·6	G.K.
			,, ,, bisection	6 5 34·4	G.K.
			,, ,, last contact	6 6 5·3	G.K.
			Eclipse, disappearance	6 16 53·5	G.K.
Feb. 1	I.	(*a*)	Eclipse, reappearance	5 44 2·5	J.C.
July 13	II.	(*b*)	Eclipse, reappearance	12 14 28·6	J.C.
	II.	(*c*)	Occult. disappearance, bisection	12 25 16·9	J.C.
19	III.		Eclipse, disappearance	14 52 26·2	H.C.
23	III.	(*d*)	Transit, egress, last contact	13 14 29·8	C.
Aug. 24	I.		Eclipse, disappearance	10 42 4·6	C.
Sept. 4	III.	(*e*)	Transit, egress, bisection	10 35 40·6	H.C.
	III.		,, ,, last contact	10 40 24·8	H.C.
10	I.		Transit, egress, first contact	9 1 31·9	L.
	I.		,, ,, bisection	9 3 1·6	L.
	I.		,, ,, last contact	9 5 1·3	L.
24	I.		Transit, ingress, last contact	10 18 43·4	D.
	I.		Transit, egress, bisection	12 27 52·5	D.
	I.		,, ,, last contact	12 31 22·0	D.
29	III.	(*f*)	Occult. reappearance, last contact	9 59 15·1	C.
Oct. 1	II.		Transit, egress, bisection	10 58 48·6	J.C.
11	I.		Eclipse, reappearance	7 48 7·0	D.
16	I.		Occult. disappearance, first contact	12 37 12·8	C.
	I.		,, ,, bisection	12 39 27·4	C.
17	II.		Occult. disappearance, bisection	7 48 19·6	D.
	II.		,, ,, last contact	7 50 19·3	D.
24	III.		Transit, egress, last contact	9 49 33·3	E.
	II.		Occult. disappearance, first contact	10 0 31·5	E.
Nov. 2	I.		Transit, ingress, bisection	7 52 47·3	C.
	II.		Transit, egress, last contact	9 20 3·1	C.
	I.		,, ,, first contact	10 0 26·5	C.
	I.		,, ,, last contact	10 3 55·9	C.

Days of Obs. 1868.	Satellites.		Phenomenon.	Solar Solar Time. h m s	Observer.
Nov. 18	III.		Eclipse, disappearance	11 27 43·2	E.
Dec. 3	I.		Eclipse, reappearance	10 15 33·5	D.
11	I.	(*g*)	Transit, egress, last contact	8 9 1·4	C.
	II.	(*h*)	Transit, ingress, last contact	8 26 28·6	C.
12	I.		Eclipse, reappearance	6 40 56·0	E.
19	I.		Eclipse, reappearance	8 36 21·3	J.C.
31	III.		Occult. disappearance, bisection	5 59 37·8	J.C.
	III.		Occult. reappearance, last contact	8 58 8·9	J.C.
1869. Jan. 4	I.		Eclipse, reappearance	6 57 46·0	S.
5	II.		Transit, egress, last contact	8 7 16·7	C.
Feb. 5	III.		Eclipse, disappearance	7 59 4·4	E.

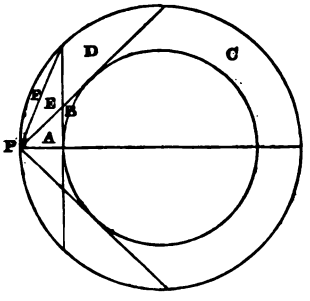
(*a*) (*d*) The image very bad.
(*b*) Uncertain, the planet being badly defined, and the satellite reappearing very near it.
(*c*) The planet very low and the obervation uncertain.
(*e*) The image very tremulous; the observation uncertain.
(*f*) Doubtful, in consequence of the planet being obscured by cloud every few moments.
(*g*) Very uncertain; the image of the planet very tremulous; clouds passing.
(*h*) Very tremulous.

The initials S., D., E., C., L., J.C., H.C., and G.K., are those of Mr. Stone, Mr. Dunkin, Mr. Ellis, Mr. Criswick, M., Lynn, Mr. Carpenter, Mr. H. Carpenter, and Mr. Keating.

Note on the Attraction of Ellipsoids.

By Prof. Cayley.

If an indefinitely thin shell of uniform density, bounded by two similar and similarly-situated ellipsoids, attracts a point P on its outer surface, it has been shown geometrically by M. Chasles that the attraction is in the direction of the normal at P, and is equal to twice the attraction of an infinite plate, the thickness of which is equal to the normal thickness at P. Assuming that the attraction is in the direction of the normal, the proof is in fact as follows:—with P as vertex, circumscribe to the interior surface a cone; this divides the shell into three parts; th



one, D + E + F, exterior to the cone, the other two, A + B a C, interior to the cone. It is shown that in the direction of t

normal the attraction of C is equal to that of A + B; and it is assumed that in comparison with these the attraction of D + E + F may be neglected; the whole attraction is thus equal to twice that of the portion A + B. At the point where the normal at P meets the internal surface draw the tangent plane to the internal surface, thus dividing the portion A + B into the solid cone A and a remaining portion B; it is assumed that in comparison with that of A the attraction of B may be neglected; the whole attraction is thus equal to twice the attraction of the solid cone A; and the attraction of this solid cone is in the limit (the aperture or solid angle then becoming $= 2\pi$) equal to the attraction of an infinite plate whose thickness is equal to the altitude of the solid cone, that is, to the normal thickness at P. And the attraction of the whole ellipsoidal shell is thus ultimately (that is, when the shell is indefinitely thin) equal to twice the attraction of the infinite plate.

It is interesting to ascertain the orders of magnitude of the attractions of the several portions of the shell, which attractions are compared in the foregoing investigation; and this can be done very easily, when, instead of the ellipsoidal shell, we have a spherical shell (bounded by two concentric spherical surfaces). The tangent plane to the inner surface divides the portion D + E + F into two portions D and E + F; and if with P as vertex we describe a cone standing on the circle in which the tangent plane meets the outer surface, the last-mentioned portion is hereby divided into the portions E and F; the whole shell is thus divided into the portions A, B, C, D, E, F, each of them symmetrical in regard to the normal or radius at P, and consequently attracting in the direction of this radius. I proceed to find the attractions of each of these portions; it will appear, in accordance with the assumptions of the foregoing investigation, that, taking the radii to be 1 and $1 + \alpha$, that is, α the thickness of the shell, and supposing ultimately α to become indefinitely small, the attractions of A and C are each ultimately $= 2\pi\alpha$, that is $=$ to the attraction of the infinite plate, while the attractions of the other portions are of the order $\alpha^{\frac{1}{2}}$, and thus vanish in comparison with that of A or C.

The attraction of an indefinitely thin cone or frustum of a cone, length r and solid angle $d\omega$ is $= r\,d\omega$; considering any such cone having P for its vertex, if the inclination of r to the radius through P is $= \theta$, and if the azimuth of the plane through r and the radius is $= \phi$, then we have $d\omega = \sin\theta\,d\theta\,d\phi$, the attraction $r\,d\omega$ is $= r\sin\theta\,d\theta\,d\phi$, and this attraction resolved in the direction of the radius is $= r\sin\theta\cos\theta\,d\theta\,d\phi$. For the several cases which have to be considered, the value of r is independent of φ, and the integration in regard to ϕ is always from $\phi = 0$ to $\phi = 2\pi$;—the attraction is thus in each case $= 2\pi\int r\sin\theta\cos\theta\,d\theta$, the expression of r in the terms of θ, and the limits of θ being known for each of the several portions of

the shell. Taking θ_1 for the semi-angle of the tangent cone, we have it clear

$$\sin\theta_1 = \frac{1}{1+a},\quad \cos\theta_1 = \frac{\sqrt{2a+a^2}}{1+a};$$

and taking θ_2 for the semi-angle of the cone which divides the portions E, F,

$$\tan\theta_2 = \sqrt{\frac{2+a}{a}},\quad \sin\theta_2 = \frac{\sqrt{2+a}}{\sqrt{2(1+a)}},\quad \cos\theta_2 = \frac{\sqrt{a}}{\sqrt{2(1+a)}}.$$

For F we have

$$r = 2(1+a)\cos\theta,\quad \theta = \theta_2 \text{ to } \theta = \frac{\pi}{2},$$

Integral is

$$= 2(1+a)\int \sin\theta\cos^2\theta\, d\theta = \frac{2}{3}(1+a)\cos^3\theta_2.$$

For D + E we have

$$r = 2(1+a)\cos\theta,\quad \theta = \theta_1 \text{ to } \theta = \theta_2,$$

Integral is

$$= 2(1+a)\int \sin\theta\cos^2\theta\, d\theta = \frac{2}{3}(1+a)_3(\cos^3\theta_1 - \cos^3\theta_2).$$

For E, we have

$$r = \frac{a}{\cos\theta},\quad \theta = \theta_1 \text{ to } \theta = \theta_2,$$

Integral is

$$= a\int \sin\theta\, d\theta = a(\cos\theta_1 - \cos\theta_2).$$

For A we have

$$r = \frac{a}{\cos\theta},\quad \theta = 0 \text{ to } \theta = \theta_1,$$

Integral is

$$= a\int \sin\theta\, d\theta = a(1-\cos\theta_1).$$

For A+B we have

$$r = (1+a)\cos\theta - \sqrt{1-(1+a)^2\sin^2\theta},\quad \theta = 0 \text{ to } \theta = \theta_1,$$

Integral is

$$= \int \left\{(1+a)\cos\theta - \sqrt{1-(1+a)^2\sin^2\theta}\right\}\sin\theta\cos\theta\, d\theta$$

$$= (1+a)\left(-\frac{1}{3}\cos^3\theta\right) + \frac{1}{3(1+a)^2}\left\{1-(1+a)^2\sin^2\theta\right\}^{\frac{3}{2}}, \text{ between the limits.}$$

$$= \frac{1}{3}\left\{(1+a)(1-\cos^2\theta_1) - \frac{1}{(1+a)^2}\right\}.$$

and subtracting the above value of the integral for A, it at once appears that, for B, the integral is

$$= 2\pi \left\{ \alpha (-1 + \cos\theta_1) + \frac{1}{3}(1+\alpha)(1-\cos^3\theta_1) - \frac{1}{3}\frac{1}{(1+\alpha)^2} \right\}.$$

Hence, calculating the approximate values, and restoring in each case the omitted factor, 2π, we have

$$\text{Attraction } A = 2\pi\alpha - 2\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

$$\text{,, } B = \frac{2}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

$$\text{,, } C = 2\pi\alpha - \frac{4}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

$$\text{,, } D = \frac{4}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

$$\text{,, } E = 1\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

$$\text{,, } F = \frac{1}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}}.$$

Or, if we please,

$$\text{Attraction } A+B = 2\pi\alpha - \frac{4}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

$$\text{,, } C = 2\pi\alpha - \frac{4}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}},$$

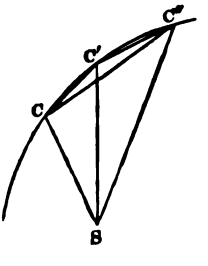
$$\text{,, } D+E+F = \frac{8}{3}\sqrt{2}\,\pi\alpha^{\frac{1}{2}};$$

so that ultimately the attraction of the portion $D + E + F$ vanishes in comparison with those of the portions $A + B$ and C; and the attraction of these last, that is, of the whole shell, is $= 4\pi\alpha$, twice the attraction of an infinite plate of the thickness α.

Note on the Problem of the Determination of a Planet's Orbit from three Observations. By Prof. Cayley.

The principle of the solution given in the *Theoria Motus* may be explained very simply as follows:—

Consider three successive positions of C, C′, C″, of a planet revolving about the focus S; let n, n', n'', denote the doubles of the triangular areas C′SC″, CSC″, and CSC″ respectively (viz. the triangular area means the area of the triangle included between the two radius vectors and the chord joining their extremities), r' the radius vector SC′; θ'', θ, the times of describing the arcs CC′ and C′C″ respectively, the units of time and distance being such that the time



is equal to the double area divided by the square root of the half latus rectum ($t = 2\pi a^{\frac{3}{2}}$ for the Period in a circular or elliptic orbit).

Then writing,

$$P = \frac{n''}{n}, \quad Q = 2\left(\frac{n + n''}{n'} - 1\right) r^3,$$

(observe that $n + n'' - n'$ is = twice the triangle C C′ C″), for neighbouring positions of the planet, the values of P and Q are *approximately* $= \frac{\theta''}{\theta}$ and $\theta\theta''$ respectively: the solution consists in the determination of an orbit for which P and Q have these approximate values; then, by means of such approximate orbit, the values of P and Q are more accurately determined, and by means of these new values of P and Q, a new determination is effected of the orbit: and so on, to the requisite accuracy of approximation.

The foregoing approximate values of P and Q respectively are deduced from the accurate values

$$P = \frac{\theta'' \eta}{\theta \eta''}, \quad Q = \frac{\theta\theta''}{\eta\eta''} \frac{r'^3}{r'r''} \frac{1}{\cos f \cos f'' \cos f'''};$$

where r, r', r'', are the radius vectors S C, S C′, S C″; $2f, 2f', 2f''$, are the angular distances C′ S C″, C S C″, C S C′ ($f''' = f + f''$) and η, η', η'', are the ratios of the sectorial areas C′ S C″, C S C″, C′ S C″, to the triangular areas represented by the same letters respectively: the doubles of the sectorial areas are thus $n\eta, n'\eta'$, and $n''\eta''$, and if the half latus rectum be denoted by p, then we have

$$\sqrt{p} = \frac{n\eta}{\theta} = \frac{n'\eta'}{\theta'} = \frac{n''\eta''}{\theta''};$$

and it thus at once appears that the accurate value of P is $= \frac{\theta''\eta}{\theta\eta''}$, as above. To obtain the expression for Q, taking $\varphi, \varphi', \varphi''$, for the true anomalies (and, for greater symmetry, writing for the moment $\nu, -\nu', \nu'', g, -g', g''$, in place of n, n', n'', f, f', f'', respectively), we have

$$r = \frac{p}{1 + e\cos\varphi}, \quad 2g = \varphi'' - \varphi',$$

$$r' = \frac{p}{1 + e\cos\varphi'}, \quad 2g' = \varphi - \varphi'',$$

$$r'' = \frac{p}{1 + e\cos\varphi''}, \quad 2g'' = \varphi' - \varphi,$$

$$(g + g' + g'' = 0);$$

whence identically,

$$\frac{\sin 2g}{r} + \frac{\sin 2g'}{r'} + \frac{\sin 2g''}{r''} = -\frac{4 \sin g \sin g' \sin g''}{p},$$

or writing

$$\nu = r'r'' \sin 2g, \quad \nu' = r''r \sin 2g', \quad \nu'' = rr' \sin 2g''$$

this is

$$\nu + \nu' + \nu'' = -\frac{4\,rr'r'' \sin g \sin g' \sin g''}{p},$$

$$= -\frac{(rr'r'')^2 \sin 2g \sin 2g' \sin 2g''}{2\,p\,rr'r'' \cos g \cos g' \cos g''}$$

$$= -\frac{\nu\nu'\nu''}{2\,p\,rr'r'' \cos g \cos g' \cos g''}.$$

This is, in fact,

$$n - n' + n'' = \frac{n\,n'\,n''}{2\,p\,rr'r'' \cos f \cos f' \cos f''},$$

or since

$$\frac{nn''}{p} = \frac{\theta\theta''}{nn''},$$

it is

$$2\left(\frac{n+n''}{n'} - 1\right) = \frac{\theta\theta''}{nn''rr'r'' \cos f \cos f' \cos f''},$$

viz. multiplying by r'^3, it is

$$Q = \frac{\theta\theta''}{nn''}\frac{r'^3}{rr''}\frac{1}{\cos f \cos f' \cos f''},$$

the above-mentioned value of Q.

On Personality in Observing Transits of the Limbs of the Moon. By E. Dunkin, Esq.

One result of the extreme accuracy of modern astronomical observations and reductions has been the detection of numerous well-marked personal differences, or discordances, which had hitherto escaped notice. One of the first discovered of these personalities, and perhaps the most constant of them all, is that existing in the determination of the absolute time of a transit of a star across the wires in the field of view of the telescope, an operation in which observers have been known to differ more than 1ˢ. Well-defined personal differences were also noticed by the late Mr. Sheepshanks in the micrometrical comparison-observations of the standard yard; similar personalities have been detected in the microscope-readings of the cross-wire bisection of a division of a graduated circle; and, lately, Mr. Stone has

pointed out a very small, but real, personal difference in the separate determinations of the error of collimation of the telescope of the Greenwich Transit-Circle, although the amount is too insignificant to affect the observations.

The personality of which the present paper treats, is one which for many years has been known to exist. For some time, however, its effect upon the observations was not investigated. So long ago as 1848, the year after the erection of the Altazimuth at the Royal Observatory, a very large personal discordance was found between the tabular errors in R.A. of the Moon observed with the Altazimuth by Mr. H. Breen and myself, by whom all the observations had, up to that time, been made with that instrument. As this comparison of results, deduced from the observations of different limbs of the Moon, is the first that I am aware of, in which the amount of personal discordance in observing the Moon's limbs, has been clearly and systematically ascertained; I cannot do better than quote in this place the statement of the Astronomer Royal on this (as it appeared to him) singular instance of observational personality.

"The circumstances under which the large errors given by Mr. Breen occurred were so various, in respect of Moon's age and Moon's position in her orbit; and the intermixture of the observations had been so complete, that there was no doubt whatever that this was the result of a difference in the mode of observation. And this was not a result of personal equation usually so called: for it was known from the investigations of personal equation, as exhibited in the clock-errors given by stars (which are confirmed by similar investigations made to the end of 1849), that the personal equation was small. Neither was it a different estimation of the Moon's diameter, for the difference of errors of Moon's R.A. is nearly the same, and in the same direction, whether the first limb or the second limb be observed. It is strictly speaking, a *difference between the personal equation for the Moon and that for the stars:* or it may be thus stated, that *the duration of the impression on the nerves of the eye, or the time occupied in bringing into comparison the impressions on the eye and on the ear, is not the same when the Moon is observed with the eye as when a star is observed with the eye.* The evidence of this is given by the following numbers.

"By observations of the first limb of the Moon from 1847, May 16, to 1848, May 28:

The mean of 45 errors of Moon's Tabular R.A., by Mr. Dunkin is ..	+0·53
The mean of 35 errors by Mr. Breen is	+0·99
Excess of Mr. Breen's	+0·46

"By observations of the second limb of the Moon through the same period:

The mean of 33 errors, by Mr. Dunkin, is	+0·50
The mean of 27 errors, by Mr. Breen, is..	+0·80
Excess of Mr. Breen's	+0·30

(*Introduction, Greenwich Observations*, 1848, p. lxx.)

A similar comparison of the errors of the Moon's tabular N.P.D. gave no sensible difference between the two observers. The Astronomer Royal fully explains the probable reason of this in the same *Introduction*.

A subsequent comparison of my observations of the Moon made on the meridian with the Transit-instrument with those of the oldest and most experienced transit-observer then at Greenwich, resulted in showing that no difference existed between us; consequently the discordance was thrown entirely on Mr. Breen's observations, to which a considerable correction was afterwards applied.

As the use of Hansen's Lunar Tables was first adopted in the *Nautical Almanac* for 1862, I had intended to make the present investigation include the seven years from 1862 to 1868. Owing, however, to a protracted illness of one of the assistants in 1862; to the absence of two assistants in the island of Valencia for a considerable period; and to the employment of another in more than the usual amount of equatorial work, the four principal observers were very unequally distributed during that year. For this reason, I have used the observations only for the six years from 1863 to 1868.

The method I have employed to exhibit the personal differences is a very simple one. My object has been to determine the yearly means, for each of the four observers, of all the tabular errors in R.A. of the Moon observed with the Transit-Circle and Altazimuth, separating those of each instrument into two groups, one consisting of all the observations before opposition, and the other containing all after opposition. As the intermixture of the observations is satisfactory in each year, particularly with the Altazimuth, the mean tabular errors become comparable for the four observers, Mr. Dunkin (D), Mr. Ellis (E), Mr. Criswick (C), and Mr. Carpenter (J. C.). To satisfy myself on this point I have, for the year 1868, arranged all the errors for each observer according to the Moon's age; the mean age agrees sensibly for all, both before and after opposition. Several observations with both instruments made by other observers I have omitted solely on the ground of the paucity of the individual numbers.

The following Tables need no special explanation, further than to state that they are only intended to give the numerical results from the data as published in the columns of the *Greenwich Observations*. That the personal discordances which they exhibit are real, there can be no manner of doubt; they are probably due principally to the different values of diameter given by each observer's observations, but how much of the personality is pre-

cisely belonging to the observations of the first or second limb is a question very difficult to answer.

The transits of the first and second limbs of the Moon, and with the Altazimuth also of the upper and lower limbs, over the systems of wires in the field of view of the telescope, are sensibly very different operations, the Moon in its progress in one case approaching the wire, and in the second case receding from it. Every person who has had any experience in this kind of observation cannot have failed to notice that the attention of the observer's mind is here occupied in performing two different modes of estimation. From this cause it is probable that the observations of the two limbs are not made under analogous circumstances, and therefore a different amount of personality may exist in each, or, in other words, the methods of two observers may be similar in the observation of one limb, and be more or less discordant in another.

Altazimuth.

In the formation of Table I. I have extracted all the tabular errors in R.A. of the Moon as determined from the Altazimuth observations. These errors have been classified into groups according to the initial of the observer's name, keeping distinct those resulting from observations made before and after opposition. The yearly means of each group are contained in the Table.

Table I.

Before Opposition.

Year.	Mean Error of Tabular R.A. by D.	E.	C.	J.C.
	s	s	s	s
1863	−0·156	−0·296	−0·139	−0·388
1864	−0·030	−0·173	−0·093	−0·363
1865	+0·005	−0·155	+0·007	−0·253
1866	+0·071	−0·059	0·000	−0·162
1867	+0·213	−0·014	+0·106	−0·057
1868	+0·162	+0·059	+0·104	−0·035
Mean	+0·044	−0·106	−0·003	−0·210

After Opposition.

Year.	Mean Error of Tabular R.A. by D.	E.	C.	J.C.
	s	s	s	s
1863	−0·073	−0·136	−0·125	−0·122
1864	+0·016	+0·014	+0·016	−0·076
1865	+0·110	+0·014	+0·041	+0·064
1866	+0·078	+0·044	+0·041	+0·119
1867	+0·241	+0·047	+0·236	+0·184
1868	+0·242	+0·176	+0·235	+0·256
Mean	+0·102	+0·027	+0·074	+0·071

By taking the differences between the corresponding mean yearly tabular errors the numbers in the following Table are obtained. If no personality existed, and if the adopted semi-diameter used in the reductions were strictly correct, these differences would have been insensible for each observer, provided the mean tabular errors are assumed to be constant throughout the mean lunation.

Table II.

Difference between the Mean Tabular Errors of R.A. of Moon determined before and after Opposition.

Year.	D.	E.	C.	J.C.
	s	s	s	s
1863	+0·083	+0·160	+0·014	+0·266
1864	+0·046	+0·187	+0·109	+0·287
1865	+0·105	+0·169	+0·034	+0·317
1866	+0·007	+0·103	+0·041	+0·281
1867	+0·028	+0·061	+0·130	+0·241
1868	+0·080	+0·117	+0·131	+0·291
Mean	+0·058	+0·133	+0·077	+0·281

A cursory examination of the numbers in the two preceding tables will show plainly that a well-marked regular personality of some kind is exhibited in each year, without exception. It appears that all the observers give a sensibly smaller error of tabular Right Ascension in the first half of the lunation than in the second half, varying from $+0^{s}\cdot06$ from my observations to $+0^{s}\cdot28$ from those of Mr. Carpenter, the difference being equivalent to $+0^{s}\cdot22$, or $3''\cdot30$. If we were to assume that these personal differences arise chiefly from different methods of estimation of the true position of the Moon's limb, and that a similar amount of personality exists in the observations of the upper and lower limbs; then the numbers in the above table would seem to show that the semi-diameter of the Moon determined by Mr. Carpenter differs from that resulting from my observations by about $1''\cdot50$. The following table is computed from Table I., by adopting for convenience the usual habit of referring all the observer's means to mine, which for the purpose we will suppose to equal 0. The numbers in the table are therefore differences of my mean results for each year from those of the other observers

Table III.

Before Opposition.

Year.	D−E.	D−C.	D−J.C.
	s	s	s
1863	+0·140	−0·017	+0·232
1864	+0·143	+0·063	+0·333
1865	+0·160	−0·002	+0·258
1866	+0·130	+0·071	+0·233
1867	+0·227	+0·107	+0·270
1868	+0·103	+0·058	+0·197
Mean	+0·151	+0·047	+0·254

After Opposition.

Year.	D—E.	D—C.	D—J. C.
	s	s	s
1863	+0·063	+0·052	+0·049
1864	+0·002	0·000	+0·092
1865	+0·096	+0·069	+0·046
1866	+0·034	+0·037	−0·041
1867	+0·194	+0·005	+0·057
1868	+0·066	+0·007	−0·014
Mean	+0·076	+0·028	+0·032

Referring to the preceding table it may be remarked that, before opposition, Mr. Ellis's mean tabular error agrees nearly with the mean of all, or is only slightly in excess of it. Mr. Criswick's mean tabular error is between the mean and mine, those of Mr. Carpenter and myself being at opposite ends of the scale. After opposition, excepting the comparison with Mr. Ellis in 1867, which from some unknown accidental cause stands out from the other years, no difference in the table reaches $0^s{\cdot}1$, while in the majority of instances the amount is below $0^s{\cdot}05$.

Although the major part, if not all, of the personality apparently belongs to the first half of the lunation when the first limb of the Moon has been observed, yet it is very probable, and even certain, that some part of it arises from other causes than by the different methods of observing the transits of the first and second limbs. In the reductions of the Altazimuth observations, the error of tabular R.A. is not directly obtained from the simple transit of the limb, as with the Transit-circle, but from a comparatively complicated series of observations and calculations, depending not only upon transits of the right or left limb, but also on transits of the upper or lower limb, in which personality creeps in as much probably as in the azimuthal transits, and upon micrometrical readings of the divisions of the horizontal and vertical circles. In vertical transits, the limb observed depends upon the position of the Moon with respect to the meridian and horizon at the time of observation. I find from a special examination of the printed volumes of the *Greenwich Observations*, that in the first half of the lunation the observations of the upper and lower limbs are nearly equally divided, and that nearly all those of the lower limb are observed west of the meridian, and nearly all those of the upper limb east of the meridian. The result of this is that, if the adopted semi-diameter employed in the reductions is too small or too great, the transits of the upper and lower limbs made on opposite sides of the meridian, would affect the resulting tabular error of R.A. in the same direction and would tend to produce, if the semi-diameter be too great, a large negative error in the first half of the lunation. After opposition, the lower limb of the Moon, with very few exceptions, is always observed east of the meridian. These complications make the discovery of the true cause of the personality be-

apparently more in one limb than another a very difficult matter indeed.

If we may suppose that the principal cause is a question of personal semi-diameter, then it would not be difficult to apply separate corrections to make the errors for both azimuthal limbs alike, but there would still be an outstanding personality left in the mean absolute tabular errors. The quantities to apply both to the azimuthal and vertical semi-diameters now in use would be small ; for each observer they would be,—

D =	− 0ˢ·02	or	− 0″·30
E =	− 0·05		− 0·75
C =	− 0·03		− 0·45
J. C =	− 0·10		− 1·50

Before opposition, as the observations of the upper and lower limbs are about equal, the lower limb being observed after the meridian, and the upper limb before the meridian, a decrease in the adopted vertical and horizontal semi-diameters tends to increase *positively* the error of tabular R.A. After opposition, a decrease in the semi-diameter in azimuth tends to diminish the tabular error of R.A., and the lower limb being nearly always observed east of the meridian, the tabular error is also diminished if the semi-diameter be decreased. I have computed, for mean hour-angle and N.P.D. the exact correction due to a decrease of 1″·50 in both the azimuthal and vertical semi-diameters.

The compound effect on tabular error in R.A before opposition is	+0ˢ·14
That due to correction of azimuthal semi-diameter	+0·09
„ „ vertical „	+0·05

After opposition the same numbers will apply with changed signs. If we apply these corrections to the means of Mr. Carpenter's observations in Table I., and proportional corrections to the means of the other observers, the individual mean tabular errors before and after opposition become accordant, although there still remains a visible personality in the absolute errors determined by the different observers.

Before Opposition.	After Opposition.
D = +0ˢ·07	+0ˢ·07
E = −0·04	−0·04
C = +0·04	+0·07
J.C = −0·07	−0·07

Transit Circle.

I have treated the tabular errors in R.A. deduced from the Transit-circle observations in a similar manner to the Altazimuth

observations. The materials are, however, much fewer, and the intermixture of the observers is not quite so complete. This comparative inferiority, with respect to numbers, is however, in a great measure compensated by the more direct manner with which the tabular errors have been obtained, and by the superior optical power of the instrument. The yearly means of the tabular errors are contained in the next table.

Table IV.

Before Opposition.

Year.	Mean Error of Tabular R.A. by D.	E.	C.	J.C.
	s	s	s	s
1863	−0·204	−0·249	−0·168	−0·249
1864	−0·071	−0·129	−0·025	−0·187
1865	+0·055	−0·072	+0·056	−0·089
1866	+0·131	+0·036	+0·147	+0·027
1867	+0·186	+0·108	+0·245	+0·101
1868	+0·220	+0·158	+0·270	+0·130
Mean	+0·053	−0·025	+0·087	−0·044

After Opposition.

Year.	Mean Error of Tabular R.A. by D.	E.	C.	J.C.
	s	s	s	s
1863	−0·111	−0·060	−0·04	−0·118
1864	+0·096	+0·060	+0·131	+0·060
1865	+0·174	+0·102	+0·168	+0·217
1866	+0·254	+0·136	+0·277	+0·194
1867	+0·350	+0·332	+0·362	+0·375
1868	+0·400	+0·323	+0·423	+0·399
Mean	+0·194	+0·149	+0·226	+0·188

The differences of each observer's mean results from the mean of all are

Before Opposition.	After Opposition.
s	s
D = +0·035	+0·004
E = −0·043	−0·041
C = +0·069	+0·036
J. C = −0·062	+0·002

By taking the difference between the corresponding tabular errors determined before and after opposition in each year, Table IV., it will be seen that in every instance the errors are in excess after full Moon. The mean quantity in excess for the six years is as follows:—

$$D = +0^{s}\cdot141$$
$$E = +0^{s}\cdot174$$
$$C = +0^{s}\cdot139$$
$$J.\,C. = +0^{s}\cdot232$$

The mean of all is $+\,0^{s}\cdot171$, showing that the tabular duration of the Moon's passage over the meridian deduced from Hansen's Tables should be diminished by about $0^{s}\cdot085$. This quantity agrees in the same direction with that deduced directly from transits of the first and second limbs at opposition, as given in the Introduction to the *Greenwich Observations* from 1863 to 1868. The next table corresponds with Table III.

Table V.

Before Opposition.

Year.	D.—E.	D.—C.	D.—J.C.
	s	s	s
1863	+0·045	−0·036	+0·045
1864	+0·058	−0·046	+0·116
1865	+0·127	−0·001	+0·144
1866	+0·095	−0·016	+0·104
1867	+0·078	−0·059	+0·085
1868	+0·062	−0·050	+0·090
Mean	+0·078	−0·035	+0·097

After Opposition.

Year.	D.—E.	D.—C.	D.—J.C.
	s	s	s
1863	−0·051	−0·107	+0·007
1864	+0·036	−0·035	+0·036
1865	+0·072	+0·006	−0·043
1866	+0·118	−0·023	+0·060
1867	+0·018	−0·012	−0·025
1868	+0·077	−0·023	+0·001
Mean	+0·045	−0·032	+0·006

With respect to the personal differences between the observers these numbers are confirmatory of those in the corresponding table in the Altazimuth results, but they are of a much smaller magnitude, as might be expected. If we place the means of the two sets side by side, their relative agreement for personality will be better seen.

	Before Opposition.		After Opposition.	
	Altazimuth.	Transit-Circle.	Altazimuth.	Transit-Circle.
	s	s	s	s
D − E	+0·151	+0·078	+0·076	+0·045
D − C	+0·047	−0·035	+0·028	−0·032
D − J.C.	+0·254	+0·097	+0·032	+0·006

The effect of the personality, whatever may be its cause, is visible in the tabular errors more in the first half of the lunation than in the second, as with the Altazimuth observations, but the differences of each observer's means from the mean of all are too small to trace the personality to any particular source. The result of this investigation has convinced me more than ever of the advantage derived from an intermixture of the observers when absolute places are required. It is now a well-known general fact that a long series of observations made by one observer may appear in most beautiful accord, and yet the mean of all may be considerably in error from the effect of some peculiar personality. On the other hand, where several observers are well intermixed, as in the observations now under discussion, personalities may exist, but their effect is most probably nearly eliminated when the results are grouped together. In personal habits of observation, it is not always easy to determine which observer approaches nearest to absolute truth, and it is very possible, and probable also, that that perfection is never attained even by the oldest or most experienced. Where several trained observers are employed in the same series of observations, it is very likely that the mean of all the observations is the correct result, and that it is nearly free from error arising from personality. But the importance of detecting the small personal differences which are found in almost all astronomical observations, and when discovered, to determine their amount, cannot be overrated. This importance must be my excuse for offering these few remarks to the Society, on a subject which is necessarily somewhat of a technical character, and which is probably interesting in a great measure to the professional astronomer alone.

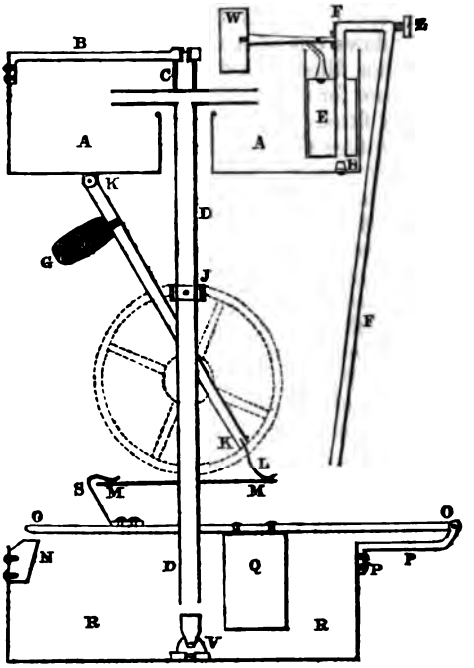
Greenwich, 1869, *April* 7.

Description of an Improved Driving-Clock.

By Sidney B. Kincaid, Esq.

The failure of the ordinary governor-clock to give in several instances which came under my own notice, when applied to Equatoreals of large size, a motion sufficiently regular for the purposes of ordinary astronomical observations, and its lack of the accuracy necessary for the successful conjunctive use of the spectroscope led me some considerable time since to give my attention to the devising of a mechanical system which should form an apparatus more nearly perfect in its performance. Many and great interruptions have hindered the task and compelled me to delay the completion of my plan. I lay it before the members of the Royal Astronomical Society now, before having it put to the test of actual experiment, because the principle in which it differs from other driving-clocks and the mode of using it can be seen, and their advantages in practice judged of and determined by

mere inspection of the scheme, and because there may be among the members of the Society some having the opportunity and the disposition to do that which I could not myself undertake for some years.



R is the lower vessel having in the centre at the bottom a conical bearing V for the lower end of the tubular shaft D to rotate in. The upper vessel A is supported over the lower one by columns, not shown in the figure, and carries at the end of an arm B fixed to its side a cylindrical bearing for the upper pivot of D. This shaft D is furnished with apertures into its interior below the surface of the fluid in the lower vessel, and with a pair of of parallel horizontal plates attached near its upper end, so that the space between these plates communicates with the bore of the shaft; the train of wheels, by which the driving-power of the weight is transferred in the usual way to the tangent-screw of the telescope, is connected with the shaft through the vertical crown-wheel, shown by the dotted outline in the diagram, which works with the pinion J on the shaft D. To the shaft is also fixed the plane horizontal concentric disk M, on which rests the steel spring S attached to the lever O which carries the float Q, and plays on a hinge at the end of the arm P fixed to the side of the lower vessel R. To the under side of A is attached by a hinge the rod

K, so that by moving the handle G upwards the spring L at the end of the rod can be made to press upon the disk M with sufficient force to stop the clock. Between guides in the upper vessel works a float E which carries the siphon F, counterposed by the weight W. H is the conical value to fit the opening of the siphon, so that when, after the clock has been stopped, the float E settles down as the fluid flows out of A through F it closes the opening and leaves the siphon filled with fluid ready for the next start. A convenient substitute for this value and the brake G K L might be found in a stopcock fitted to the longer limb of the siphon. Z is a screw to adjust the flow through the siphon. N is a stop for the end of the lever O.

Suppose now the weight wound up, the siphon filled with fluid, and the disk M relieved from the pressure of the spring L. By pouring fluid into the lower vessel R the float Q will be made to rise with the lever O, thus freeing M from the pressure of the spring S. The descent of the weight will now make the shaft D rotate rapidly, and the centrifugal force developed will exhaust the fluid from the lower vessel R and pour it into the upper one A, from which it will flow back through the siphon F into the lower vessel. Now adjust the screw Z so that when the clock goes at the proper speed the flow through F may be exactly equal to the flow through D. Then, since the velocity of the flow of a fluid under a constant pressure through an orifice of constant size is very nearly constant, and the float E keeping the end of the siphon always submerged to the same depth makes the pressure of the column of fluid in F constant, it follows that the rate of flow through F will always be what the rate of flow through D ought to be. If the speed of the clock is increased more fluid would flow through D than through F, and thus its level in R would fall, and the float Q sinking with the rod O would cause the spring S to press more hardly on M and to thus diminish the speed; if, on the other hand, an increase of resistance in any part retarded the clock, more fluid would flow through F than through D, and the float Q would rise and diminish the pressure of the spring S on M and increase the speed.

It is to be noticed that in this apparatus the regulator is perfectly independent of the driving-train, that every variation from the normal rate is instantly and *absolutely compensated*, and that in constructing such a clock the sensitiveness can be increased by increasing the normal flow.

The greater mobility of a fluid than of any system of mechanical joints made me determine, in the first instance, that it was by the use of a fluid that I should endeavour to attain the object I had in view. Whatever prejudice there may be against the use of a fluid for the purpose is unreasonable, forasmuch as the only objection which could be rationally urged—its liability to freeze, and thus to render the apparatus useless for the time being during the colder seasons of the year—does not apply either to the fluid I myself intended to be used—castor-oil, selected from specimens

which are not congealable by the winter cold of this country, and such are readily to be met with in commerce—or to that which Mr. Huggins suggested to me, glycerine.

Trinity College, Cambridge,
27 March, 1869.

On the Practical Speed of Electricity through 7200 Miles of Land Wire. By G. Davidson, Esq., Astronomer, United States Coast Survey.

(*Communicated by C. Piazzi Smyth.*)

Observations to the above end were made on the night of Feb. 28, 1869, at San Francisco, California, on a loop of wire extending thence to Cambridge, Massachussets, 3600 miles, and returning to San Francisco; the return signal being received on a chronograph close to that receiving the outgoing signal.

The total interval of time between the records of the two signals amounted to 0·8 of a second; and this may be called the *practical* speed through 7200 miles of land wire, because that length was cut up in the usual service manner of telegraphic companies, into 11 portions, say of 650 miles each; these portions being connected by as many relays (or repeaters, as they are called in America), which send forward on each of these occasions a new electrical current into the next section of the wire; and such new electrical current would travel through its new 650 miles of wire much more quickly than an original current through the second, third, or any subsequent continued length of 650 miles.

The integral result may, therefore, be looked on as a safe and satisfactory multiplication by 11 of the time occupied by electricity in both travelling through 650 miles of land wire, and working *one* relay of the kind, whatever that may be, now used in the United States; the time occupied in effecting such work being apparently 0·07 of a second.

Instrument for Sale.

A very fine Equatoreal, by Cooke of York; almost new. Aperture 6 inches, clock movement, eye-pieces, finder, &c. Also, a Sidereal Clock and Chronometer, by Dent. Address Miss Naylor, Altrincham.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *May* 14, 1869. No. 7.

Admiral MANNERS, President, in the Chair.

Dr. F. Brünnow, Astronomer Royal for Ireland, Dublin;
Rev. Robert Crowe, Huddersfield;
Thos. Cooke, Esq., Southampton Street and York;
John Knox Laughton, Esq., Royal Naval College, Portsmouth; and
William Locke Lancaster, Esq., Down's Park Road, Hackney,

were balloted for and duly elected Fellows of the Society.

Note on an Aurora Borealis, April 2nd, 1869.
By John J. Plummer, Assistant at the Durham Observatory.

A short time since the idea occurred to me that something might be added to our knowledge of the nature of the Aurora Borealis, by means of the analysis of its light by the spectroscope, and I resolved to examine its spectrum upon the first opportunity. A very fine Aurora was visible at Durham, soon after midnight on April 2nd. It is the result of the observations made upon that occasion, that I have now the honour to communicate to the Society.

The light of the Aurora appeared to the eye as white, perhaps slightly bluish, and not of the more common ruddy hue; there were few or no rays, but broad sheets or waves of light succeeded each other rapidly, proceeding from the arch to a height of about 30° from the horizon. When the light was examined by the spectroscope, one *bright* line was immediately seen. At the brightest parts of the arch, this line was perhaps equal in intensity to that of the most conspicuous of the lines in the Nebula of *Orion*, it presented ordinarily a somewhat hazy appearance, but at times would flash out sharply and well defined. It was difficult to make any accurate measurement of its position, but two approximate mea-

sures were taken, by illuminating the field suddenly, by a hand-lamp and placing the cross-wires as nearly as possible on the position in which the line had been seen. These two estimations are respectively $19^{rev.}\cdot40$ and $19^{rev.}\cdot25$.

The following are the readings of the principal Solar lines, as measured on March 6th, 1869, for referring these numbers to their places in the spectrum:—

rev.	rev.	rev.	rev.	rev.
A = 14·909	*a* = 15·397	B = 15·934	C = 16·496	D = 18·000
B = 19·961	*b* = 20·357	F = 21·751	G = 25·307	H = 28·503

The two conspicuous pairs of lines in the air spectrum near D, would be represented on this scale by the numbers $17^{rev.}\cdot89$ and $18^{rev.}\cdot61$; but I cannot believe that the two aforesaid estimations are so much in error that the latter of these lines is the one seen in the spectrum of the Aurora. I am, therefore, led to the conclusion that the spectrum of the Aurora is *not* identical with that of air; but as some doubt may exist on this point, I shall anxiously await the next apparition of an Aurora to verify this result. The advanced season, however, renders it unlikely that an opportunity will occur for some months. I have, therefore, thought it right to communicate my present results, however imperfect they may be.

It may be worthy of remark that the line in the Aurora spectrum agrees closely with that of the more conspicuous of the lines in the spectrum of *α Orionis* between the solar lines D and E. I have measured this line upon two occasions, Feb. 12 and Feb. 13, and the resulting measures were $19^{rev.}\cdot350$ and $19^{rev.}\cdot381$ respectively. There is also a tolerably conspicuous line in *Aldebaran*, near the same place. It may be also necessary to state that all these measurements have been referred directly to the sodium line D, by the observation of the spectrum of the metal from the salt with an alcohol flame, both before and after the observations. They are thus strictly comparable.

Durham Observatory,
April 6th, 1869.

Addendum.

Since the above paper was written, I have been informed by Mr. Huggins, that a similar observation has been made by Angström, and confirmed by Struve, during the winter 1867–68. I was totally unaware of this at the time of communicating the above paper. I will add that I have also had an opportunity of confirming my own observation. An aurora of surpassing magnificence was visible in the North of England upon May 13; a corona was formed, and every part of the sky for a short time was filled with streamers. Some of these, in the West and South-east especially, were of a deep red colour, and the single line in

the spectrum was seen here precisely as in the white parts of the aurora. I succeeded in making one good estimation of the position of the line from the red streamers, sufficient to identify it with that of the other measures made later on the white arch which was formed in the North-west after the corona and streamers had partially or wholly disappeared. This estimation gave $19^{rev}\cdot18$ as the position of the line. The subsequent measures give mean results of $19^{rev}\cdot03$ and $19^{rev}\cdot10$ respectively, which are believed to be tolerably close approximations, though not quite so satisfactory as had been hoped for.

No other lines were seen.

The extreme tenuity of the matter forming the aurora was demonstrated by the fact that Winnecke's Comet was visible with the $6\frac{1}{2}$-inch refractor of the Observatory, through one of the densest streamers, without any other inconvenience than that of the brightness of the field of view.

Durham Observatory, May 17th, 1869.

On the Determination of Longitudes by Electric Telegraph. By E. D. Ashe, Commander R.N., Director of the Observatory, Quebec.

As I have had some experience in determining the longitude by electric telegraph, and, as it is likely to be useful to several of our Members in different parts of the world, I send an account of fixing the position "Rivière du Loup."

Sir William Logan (Director of the Geological Survey) requested me to obtain the latitude and longitude of "Rivière du Loup," which is about 130 miles below Quebec.

I started by railway on the 4th February; took with me a 24-inch transit; and a sidereal chronometer.

On my arrival, I called on the telegraph operator, who I found could not read by sound, and, consequently, I resolved to fix my transit-instrument in his garden, so that I should have only to take the chronometer into his office when I was ready to send signals.

I generally select a boulder, or get a large stone, upon which I place the instrument; erect a hut to keep the wind off; lead the telegraph wire into it; and, with a little portable telegraph-instrument that I always carry with me, I have all that is required when I am ready to send signals.

In this instance all the Earth was covered with snow for several feet in depth, and nothing like a stone for the support could be obtained; so I got a flour-barrel, removed the snow from the earth, and placed the barrel on the ground; then filled it with sand, poured two or three buckets of water over it and around it, and, as it was freezing, placed a square piece of board on the top; and as the thermometer was several degrees below

zero, in a few minutes the whole was one solid mass. I then with boards built a hut.

I had to wait several days for a clear night, when I got the instrument into the meridian. As the country was covered with snow, and as all the fences are rails, you are sure of getting a "meridian mark" in some of the many posts. I had one about four miles off, a black line upon a white ground.

I have frequently been annoyed by boys, particularly in Upper Canada, where they are of the English type, who, when they see a light, commence throwing stones, and it is impossible to count seconds under those circumstances. On this occasion I heard one night a boy's voice, saying, "Please, sir, will you let any body in?" "No! but you may look through the crack." I cannot say if he took advantage of my kind offer; but if he did, he was as quiet as a mouse.

I got both latitude and longitude in the first four clear nights. After I had sent "time" to Quebec, I removed the transit-instrument, and drew a circle through the three points made by the three supports of the instrument, and moved one of the points 90° on the circle, and then I had the instrument on the prime vertical for latitude.

I was detained 19 days at the hotel waiting for clear weather. The expenses for travelling, building hut, paying operators at both ends of the line, amounted to 13*l*. This does not include remuneration for myself.

Quebec sent "taps" from the "mean time clock," commencing at the even minute, and continuing beating seconds until the 50 second in each minute, and this they did for six minutes. "Rivière du Loup" noted the "coincident tap" with the sidereal chronometer, marking the second and the series; and thus we had the difference of longitude to the hundredth of a second at once.

"Rivière du Loup" then sent "sidereal seconds" to Quebec, and this was repeated twice from each end of the line.

I cannot help thinking that this plan is the easiest and least troublesome, only requiring the telegraph line for half an hour, which is of great consequence.

The Latitude by Transit in Prime Vertical.

	°	′	″
α Aurigæ ..	47	49	44
1071 B.A.C. ..	47	49	45
			89
Latitude of Rivière du Loup	47	49	44·5

At Quebec 11 stars were observed for clock error,—7 stars, lamp west, and 4 stars, lamp east; deviation obtained by *Polaris* and ε *Ursæ Minoris*, and level frequently applied.

At Rivière du Loup 13 stars were observed,—7 lamp east and 6 lamp west. Deviation by *Polaris* and ε *Leporis;* and level frequently applied.



1st Set of Signals from Quebec.

	h	m	s
Coincident tap sent by Q. at ..	8	52	10·00
Clock fast			−45·37
Mean Time at Q.	8	51	24·63
Ditto at R. du Loup	8	58	3·37
Diff. Long.	0	6	38·74
Coincident tap noted at R. du Loup	5	18	35·00
Clock slow		+ 1	1·71
Sid. Time	5	19	36·71
Ditto at Mean Noon	20	20	5·04
Ditto since Mean Noon	8	59	31·67
Retardation		1	28·30
	8	58	3·37

1st Set from Rivière du Loup.

	h	m	s
Coincident tap noted by Q. at	9	28	5·00
Clock fast			−45·37
Mean Time at Q.	9	27	19·63
Ditto at R. du Loup	9	33	58·38
Diff. Long. 2d Set		6	38·75
,, 1st Set		6	38·74
Coincident tap sent by R. du Loup	5	54	36·00
Clock slow		+ 1	1·71
Sid. Time at R. du Loup ..	5	55	37·71
Ditto at Mean Noon	20	20	5·04
	9	35	32·67
Retardation		1	34·29
	9	33	58·38

The next two sets of signals were turned into sidereal time, giving —

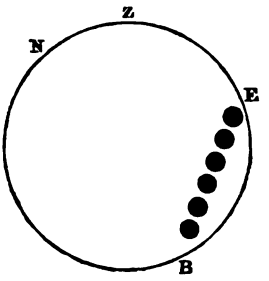
	h	m	s
A Mean Difference of Longitude of	0	6	38·70
And the Grand Mean of the 4 Sets was	0	6	38·725
Longitude of Quebec	4	44	49·02
Longitude of Rivière du Loup ..	4	38	10·295

Observatory, Quebec, March 4, 1869.

On the Transit of Mercury over the Sun's Disk on 5th Nov. 1868, *as seen at Vizagapatam.*

(*In a Letter from A. V. Nursing Row, of Vizagapatam, to Prof. C. Piazzi Smyth, Edinburgh.*)

The annexed diagram represents the disk of the Sun at the time of the Transit, as visible to an observer at Vizagapatam, for direct image.



Z represents the vertex, or the highest point of the Sun's disk.

N the north point.

B the point of first contact.

E the point of last contact.

	Mean Time. h m s
First contact	10 58 39
Last contact	2 35 57

The dark spots in the diagram represent the dark disk of *Mercury* when crossing over the Sun's disk.

"I have the pleasure to enclose herein a copy of a printed notice to friends in this neighbourhood, containing a rough diagram of the Transit of *Mercury* as it was seen here in Vizagapatam. The weather certainly favoured us on the 5th of November, and we enjoyed well the observation of the Transit. The planet was seen as an intensely dark spot on the Sun's disk. When the planet approached half-way of the transit, some of my European friends and myself observed a wavy tint of light darting from the upper edge, disturbed at times, but continued until the planet had passed some distance from the highest point of the line of transit.

"Suspecting it to be an effect of disturbed focus, I often refocussed the instrument and changed the eye-pieces, besides other precautions, yet we always observed the same phenomenon, but with high powers we found the edge of the planet was not so intense as in the centre, and especially so at the line of light in question. The telescope we used was the one which I explained in my pamphlet on the recent solar eclipse, and shows the stars without wings, &c., therefore I am very anxious to be enlightened on the cause of such appearance, if it were not in any way an optical delusion.

"A scientific friend who was then present remarked to me that he was informed somewhere that the planet, when leaving the Sun's disk, would have the appearance of a flask. I tried my best and watched for it, but did not find with my instrument anything but a dent on the edge of the Sun's disk at the last contact."

Note by C. P. S.—The writer of the above extract is a Hindoo gentleman of Vizagapatam, whose family has been much given to science through two generations. He possesses an extensive Observatory, both astronomical and meteorological, and is now about adding to the former a 6-inch Equatoreal, with driving-clock, micrometer, and spectroscope; and to his library all the purchasable volumes of the *Monthly Notices of the Royal Astronomical Society*.

The telescope with which the above observations were made has an object-glass of 4·8-inches aperture, by the late W. S. Jones, of London, and was fitted up in Mr. Nursing Row's own workshop on a large and original kind of altazimuth stand, supplied with vernier-circles, levels, and slow-motion screw-movements. In his solar-eclipse pamphlet of last August, Mr. Row thus alludes to the telescope:—

"As to the character of the object-glass of the present telescope I may add that it gives no colour around the image of any object; and that the image of any fixed star it shows is free from the optical appendages of wig or wings. It can be judged from this that the present telescope will discharge its duties faithfully."

The failure of Mr. Row to see the irradiation-distorted figure of the planet *Mercury* is probably due to his appearing, from his description, to have expected to see it, not at the internal, but at the external contact of Sun and planet. What, however, the *wavy* phenomenon was which he saw so persistently during the middle of the transit I do not pretend to suggest any explanation of.

On a Method of Imitating the Transit of an Inferior Planet.

By H. W. Hollis, Esq.

At the close of the November Meeting of the Royal Astronomical Society, I mentioned to the President an experiment which I had made with a view to obtaining artificially the appearances presented immediately after the first or before the last internal contact at the transit of an inferior planet. I was led to make this experiment from having observed, at the transit of *Mercury* on the 5th November last, the formation of the black connecting band or ligament between the planet and the Sun's limb, immediately before the last internal contact. On that occasion I found it quite impossible, on account of the formation of such apparent connecting band, to say within several seconds when the real internal contact took place.

At the top of a tower 150 feet high, I placed a projecting spar of wood, and from the outer end of this spar I suspended by a fine wire a lead ball, half an inch in diameter. I then went to a position upon the ground, 286 yards distant from the ball, which, from that position, would subtend an angle of about 10 seconds, or nearly the apparent diameter of *Mercury* at transit.

I waited until, by the effect of the Earth's rotation, the ball was seen projected upon the Sun's disk, and upon then observing the ball with a 6-inch refractor, and power 220, I obtained, at its passing off the disk, an exact representation of the formation of the black connecting ligament. Of course, the duration of this artificial transit was very short, and the apparent motion of the ball across the disk, or rather, of the disk behind the ball, was such as to render the approach of the ball to the limb infinitely more rapid than the corresponding appearance in the real phenomenon, and the ligament was only visible for a second or two, but still it was sufficiently distinct to render the experiment an interesting one.

On mentioning this experiment to the President, I was requested by him to give a written description of it, to be laid before a subsequent Meeting; and I now do this the more willingly from having observed that the possibility of making such an imitation of a transit was referred to at the last Meeting.

Keele, Newcastle, Staffordshire,
5th May, 1869.

Note on the Preparations desirable for Photographic Observations of Phenomena such as Transits of Venus. By Major Tennant.

Mr. De La Rue has in the *Monthly Notices* for December last again brought forward the claims of Photography to be used in permanently recording for examination at leisure the phenomena of the Transits of *Venus* in 1874 and 1882. I believe the probable value of this method is very great, though I feel that evidence has still to be adduced on this subject before it can be unreservedly trusted; and I propose here offering some suggestions as to apparatus and procedure, in the hope of inducing a fair trial.

First. It appears to me that an effort should be made to get rid of instrumental distortion; for I think the necessity for investigation of its amount and of applying corrections a serious drawback. If a reflecting telescope with *unsilvered* glass mirrors were used, the light would be very much diminished; and still more so if the image were optically enlarged. In a Newtonian telescope this might be done by either a convex or concave lens achromatised for the actinic rays, of which arrangement the latter would make the apparatus more compact; or the telescope might be a Cassegrain, thus becoming still more compact and manageable. An instantaneous shutter would in any of these cases allow a fairly large aperture to be used, and this having its centre part removed would give good definition.

Next. I would make a modification in the way of releasing the shutter. At Kew it is held against a spring by a thread which is cut with scissors. I would propose that it be retained by an electro magnet, and that the current forming this should also pass through



a chronograph. If an observer at a separate telescope had a break circuit key he could at any moment photographically record a phenomenon he saw and the instant of its occurrence. An assistant at the heliograph would be needed to replace the shutter and insert the dark slides, unless the observations are far apart. By using a repeating slide and combining the measurements on one plate, very valuable normal relations of the Sun and planet would be found, and observations might be made during the ingress or egress (possibly two of the black drop) in considerable numbers.

There is an advantage in this mode of observing the transit of *Venus* to which Mr. De La Rue has not alluded.* If accurate micrometrical observations can be made by means of photographic pictures, then the range of suitable stations can be greatly enlarged; for any two stations, 140° or 150° apart, can have the observations combined by choosing a suitable time, and, of course, by means of equations of condition, the observations of stations in all sorts of places could be used with their proper weight. All the photographs could be measured by the same micrometer, whose errors of all sorts could be very carefully examined.

Before, however, it could be expected that reliance should be placed on these photographic results, it is clearly necessary that a rigorous examination should be instituted of the forms of apparatus &c. An image formed, as that in Mr. De La Rue's apparatus at Cranford, or mine at Guntoor, is evidently almost free from distortion, save those arising from the collodion film. It is small, and would admit of measurement to great accuracy with a micrometer-screw of fine pitch. If enlarged by any of the means before described, coarser means of measurement would produce results equally accurate, but the distortion of the enlarging lens would be involved. Lastly, the small images might be enlarged by a specially made lens (as in the case of the Guntoor photographs), and these might be measured. In some respects this method would be better than the last, as the original images might be better centred in the axis of the enlarging lens, which, too, would be of longer focus.

All these methods must, to produce confidence, be tried. Probably as convenient a method as any would be to take photographs in various ways of any nearly total eclipse, and to treat them rigorously as micrometrical measures would be treated in an observatory, deducing the probable errors of one observation in each way.

I believe (for my own part) that the results would prove photograph registration to be thoroughly reliable as a substitute for

* Mr. De La Rue has pointed out the facility with which the nearest approach can be got from the photographs at any one station, but if photographs be taken at two stations at a time when *Venus* is in the plane which includes them both as well as the Earth's centre, these will show the whole effect of parallax; and it is the positions at these instants, and not the nearest approaches to the Sun's centre, which should be compared.

micrometrical measurements in the ordinary way, and possibly they might be much better. Even if the suitability were disproved, it would be of some value and might save a waste of energy during the transit of *Venus*.

If it be determined to send parties to photograph during the next total eclipse in the south of India, then a Photoheliograph might be placed at the centre as well as at either edge of the shadow-path as well as at the apparatus for the total phase only, and a very complete examination of the question at issue would be possible.

Mooltan, March 22, 1869.

Comments on Major Tennant's Note on the Preparations for Photographing the Transits of Venus. By Warren De La Rue, Esq.

I beg leave to offer a few remarks on some points in Major Tennant's communication.

1st. With respect to the choice of instruments; a reflecting telescope with unsilvered mirrors would certainly present the advantage over chemically achromatised refractors, in which, as in the Kew Photoheliograph, the image is enlarged by a secondary lens, of giving an image free from optical distortion; that is to say, the Cassegrainian and Gregorian present these advantages, but not so the Newtonian, if an achromatised lens is to be used to enlarge the image before it falls on the sensitive plate. My experience has proved to me that the necessity for obtaining a flat field so complicates the problem that hitherto no lens has been made which answers the conditions to be solved.

Now with respect to the Cassegrainian and Gregorian forms of instrument; it is of course necessary to make a provision for the free transmission and dispersion of that portion of the light which passes through the mirrors; they would have necessarily to be supported in cells open at the back, and their back surfaces would be advantageously made concave (convex towards the first or reflecting surface), in order to prevent any light being reflected by the back of the mirrors and forming a secondary image. The heat emerging from the back of the principal mirror would seriously interfere with successful photographic manipulations, and, although it is conceivable that special provisions could be made to diminish this inconvenience by reflecting aside the transmitted rays, it would complicate the apparatus.

By employing a Newtonian without an enlarging lens the image would be considerably smaller, but it would be free from optical distortion, and by the use of a Herschelian prism the light would in this case be reduced by a second reflection from glass (as in the other forms of reflectors), but the operator would be clear of any of the refracted heat rays, and would work at ease. I give therefore, the preference to the Newtonian.

I do not think, however, that the reflector is on the whole so convenient an instrument for observing transits of *Venus* as a refractor on the Kew model; for the optical distortion of such refracting instruments can be ascertained practically and allowed for, without in any way interfering with the final correctness of the deduced results.* Moreover, strictly speaking, the reflector is not absolutely free from optical distortion on account of the photographic plate having a flat and not a curved surface.

2nd. As regards the measuring instrument I am afraid that I have failed to convey a correct idea of the capabilities of the micrometer I devised for measuring the eclipse-pictures of 1860, and which I have described and figured in the *Phil. Trans.* of 1862, pages 373–380. Major Tennant evidently thinks that the instrument is only calculated to deal with quantities no smaller than the $\frac{1}{1000}$th to the $\frac{1}{2000}$th of an inch, representing when used in conjunction with the Kew Photoheliograph 0″·5 to 0″·25; and that the substitution of a micrometer screw for the scale of equal parts and Vernier would be an improvement. The instrument is really provided with a micrometer screw, which may be used for making the measurements of any required minuteness independently of the scale of equal parts, which was added for the special work in hand, namely, for the measurement of pictures already enlarged by the secondary magnifier to four inches in diameter. The clamping apparatus for throwing the micrometer screw in and out of action is also a special provision for the measurements of the actual Sun pictures, for it would be exceeding fatiguing to give several hundred rotations to the screw whenever a measurement had to be made. There really is no limit to dimensions of the instrument, which is adapted for the measurement of any rectangular or other co-ordinates or any angular quantities which may be desired, and by increasing the optical power of the microscope it may be used for measuring astronomical photographs with any degree of precision for the special requirements of the case. It would be quite unnecessary, and indeed detrimental, to enlarge photographs for micrometrical measurement when once they have been procured by any form of heliograph.

3rdly. With respect to the localities to be selected, the employment of the photographic method of observing transits of *Venus* has, as I have stated in the *Monthly Notices*, vol. xxix. No. 2, page 49, the advantage of rendering us independent of conditions favourable or indeed essential for eye-observations; for a few photographs obtained at each station would afford the means of ascertaining the path of the planet and its position at any given moment; and hence the proposal to determine the position of the planet's centre in relation to the Sun's centre really includes the particular case contemplated by Major Tennant in the foot-note appended to his paper; but it is possible that the Sun might be obscured at the critical time, at one of the two stations selected.

* *Monthly Notices*, vol. xxix., p. 51.

I did not describe the method of ascertaining the position-angle of the planet (at p. 51 of *The Monthly Notices*, vol. xxix.) because the means of determining the position-angle of wires in the foci of the primary and secondary lenses of the Kew Photoheliograph has been fully described in the *Phil. Trans.* for 1862. I may mention, however, that it can be ascertained within 1′ of arc.

On the Solar Eclipse of 1871. By Major Tennant.

(*Extract from a Letter to Mr. De La Rue.*)

I sent last mail to Mr. Airy a proposition to superintend three parties in the south of India in the Eclipse of 1871. I fear he will think it far too extravagant for recommendation. The idea of three parties is I believe yours; I was sorry there were not three last August, but in that stage of the matter it was hopeless, and the point I lay most stress on is that each party who go to photograph the obscuration should have a Photoheliograph to make use of, and to try rigorously by purely astronomical tests. A small probable error for the measures from a picture would be conclusive, unless it was shown that all would have a common error. Now if two pictures could be taken on one glass, these *could not have* any common error. If the pictures were taken in the principal focus of the telescope there would be no distortion except a very minute amount arising from equal parts of the image not corresponding to equal angles. This could be corrected (if needed at all) by mere theory.

I have not added in the proposition what is I think important, and that is, an examination of the Corona Spectrum with a large telescope, and arrangements to show whether it has any lines. The cost of this would be a good deal. The telescope *would not need equatoreal mounting* if an assistant were available, but a spare assistant would probably be equally costly, and he would need, as in M. Janssen's apparatus, a good large telescope himself. I should, too, like to be further on with other things before looking at this. I should not undertake it (if I had three parties) till I were sure of the leaders at all events.

I wish I could give some estimate, but that I cannot. Using chronometers and leaving out clock &c., would save, but I should prefer the other plan, and I do not look to it in any sense as all money spent on the Eclipse, for a large part would clearly be money spent in explanatory experiment as to the applicability of Photography in 1871 to delicate astronomical purposes.

If three Photoheliographs at stations near each other, whose absolute and relative place are well known, give accordant values for the tabular errors of all sorts, and assign a small probable error to one observation or photograph, then I think the matter will be settled beyond dispute, and I believe, if proper care be taken, the result will be decisive.

I cannot be at home to start it, and must hope that some of

my friends will urge the matter. You have yourself taken so much interest in the application of Photography that I know you will aid if you approve of the means. If it is to be carried out by *me* I should be got to England as soon as possible, and I certainly say I shall have no objection to an early start. Three months, however, would probably elapse between the Secretary of State making up his mind and my reaching England, and that is a very sensible portion of what is left for preparations.

On the Solar Eclipse of August, 1869.

By R. T. Paine, Esq.

(*Extract from a letter to the Secretaries of the Society.*)

You are aware that on the 7th of next August the shadow of the Moon will pass over North America, and that, on that day, in some of our *Atlantic* States there will be a total eclipse of the Sun. It will be the fourth within ninety-one years, and the third in this century, and there will be only one more before 1901 (May 28th, 1900), unless we include Texas in the *Atlantic* States, over which the shadow will pass in the fourth return of our great eclipse of June 16, 1806, on July 29th, 1878.

As such great interest has of late years been taken in these phenomena, and as the eclipse of next August is the first return of the one so successfully observed by many English astronomers, in July, 1851, in Sweden, it has seemed to me as very probable that some of them would visit North America for the purpose of observing also the one now approaching; hoping that this opinion may prove correct, I take the liberty herewith to transmit a computation of the path of the centre of the Moon's shadow, and to make the request that you would deliver it to any one who may contemplate undertaking this voyage, and to whom it may be acceptable. This computation was made by myself some months ago, for my own information and has not been made public. It was made from the longitude, latitude, and other elements of the Sun and Moon, as very carefully deduced from the English *Nautical Almanac*, except that for Encke's Solar Parallax on August 7th, the quantity $8''{\cdot}83$ was used.

The duration of totality when longest (near Mount St. Elias) will be quite four minutes, but it gradually diminishes to $3\frac{1}{2}$ ($3^m\ 28^s{\cdot}4$) at our boundary line, latitude 49°; at the City of Des Moines, the capital of Iowa, it will be exactly three minutes; at Springfield, the capital of Illinois, $2^m\ 51^s$, and about 30 miles S.W. of Raleigh, North Carolina, $2^m\ 27^s$. The territories of Montana and Dakota are we believe, like British America, on the central line, thinly inhabited, but Fort Union on the Missouri (which is reached by steamboats from St. Louis) appears by the map to be very near that line, and, therefore, if correctly laid down, is favourably situated, also in Iowa, the cities of Des Moines, Boonsboro', and Burlington, are all under or very near

that line; and although distant 1400 or 1500 miles from us, are reached in three days, on continuous railroads from this city and New York, whilst Macomb and Springfield in Illinois, New Albany, in Indiana, and Louisville in Kentucky, equally well situated, and at the distance of about 1000 miles, can be reached in two days, and Abingdon, in Virginia, and Raleigh, in North Carolina, in one day from Washington City, from which they are distant less than 400 miles by railroad.

Now that we are told it is not impossible to see the red flames around the Sun, even when it is not eclipsed, it seems strange they have not always been seen when the obscuration by the Moon was complete. Indeed, the first good account of them I had from an actual observer, was only 18 years ago, from my lamented friend, the late Prof. G. P. Bond, who saw them in Sweden during the totality of July, 1851. I believe I may say they were not noticed by the late Dr. Bowditch, who observed with great care the eclipse of 1806, which occurred here, on a beautiful day in June, with a totality of five minutes, only half an hour from noon, and certainly I did not perceive them in the total obscuration of November 30, 1834, at Beaufort, in South Carolina, where it was central. The duration of totality was to be sure only little more than a third of that here in 1806 ($1^m\ 49^s{\cdot}6$). The second contact I closely watched through my telescope with a power of 40, and a double (red and green) screen, but saw no flames; as soon as possible I read off, and recorded with a lantern the time by a chronometer, and then with an opera-glass looked at the corona and saw that magnificent and appalling spectacle, and at the same time the star *Antares*, that day in conjunction with the Sun, and therefore distant only by its latitude, 4½ degrees. The sky was very clear throughout the eclipse, and the edges of the Sun and Moon sharply defined, and without any apparent distortion whatever.

So much was said last year in the magazines and newspapers about the length of totality ($6^m\ 48^s$) where the eclipse of August 18 was central at noon, which was called "unequalled in centuries past and to come," that merely from curiosity I was led to look into the subject, and the result of my examination thereof was published in a communication in one of our papers, a copy of which I take this opportunity to transmit to you. Since last August I have carefully recomputed, by the tables of Carlini and Burckhardt, but using the Greenwich semi-diameter of the Sun, and Hansen's of the Moon, the second and third contacts of the eclipse of August 7, 1850, for latitude 17° 50′ N. longitude 141° 49′·4, W. where it was central on the meridian, and found they took place as follows:

	G.M.T.	L.M.T.	
	h m s	h m s	
Second contact	9 22 55·3	11 55 37·7	A.M
Apparent Conj. of ☉ and ☽ in Longitude	0 26 50·9	0 59 3·3	
Third contact	0 29 46·7	0 2 29·1	P.M.

	Moon's Ap. Long. — Sun.	Moon's Ap. Lat. — Sun.	Moon's Ap. S.D. — Sun.
	′	′	′
Second Contact	−72·98	+3·60	+73·07
Ap. Conj. of ☉ and ☽ in Long. ..	0·00	+0·23	
Third contact	+73·01	−3·14	+73·08

Altitude of Sun and Moon at meridian 88½°; duration of totality, 6ᵐ 51ˢ·4, or 3½ˢ longer (if I am correct) than in August 1868. The matter is, however, one merely of curiosity, and may, therefore, be considered as of little importance. I think, however, we may say that in one or two former returns of the series of eclipses to which that of last August belongs, the duration of totality, where central at noon, was quite as long as it was then.

The rapid increase of interest in astronomical phenomena is highly gratifying; perhaps there is no stronger evidence of the rapidity of this increase than that afforded by the degree of attention paid to several of the last, and to the last returns, of the above series of eclipses, which is remarkable, indeed, for the length of totality, where these eclipses are central. The returns of June 20, 1778 (central in United States), and those of July 1796 and 1814, took place, it is believed, with little if any notice; and although at the return of July 27, 1832, only thirty-six years ago, the Moon's shadow passed at about 8 A.M., over two of those quite *near* islands, the Bahamas, viz. Great Inagua and Grand Turk (the latter in N. latitude about 21½°, W. longitude 71½°), and caused a totality of quite five minutes, it did so, it is supposed, without a single scientific observer. In February of the preceding year I saw in the south-eastern part of this State, the first of the four *ring* eclipses I have witnessed, and was thereby rendered desirous of beholding a total obscuration, but the want of a direct communication induced me to forego that pleasure at that time; probably the same difficulty was experienced by those European astronomers who wished to go to Africa or the Bahamas for the same purpose, and the difficulty was then considered insuperable, but it would have easily been overcome by a small part of the zeal and enthusiasm displayed by the observers in India in August, 1868.

Path of the Centre of the Moon's Shadow over the Earth, Saturday, August 17, 1869, *according to Elements of the Sun and Moon as given by the Tables of Le Verrier and Hansen in the English Nautical Almanac, except that for Encke's Parallax of the Sun on August* 17, 8″·83 *was used. Ellipticity of the Earth* $\frac{1}{300}$*th.*

	G.M.T.	L.M.T.	Latitude.	Longitude.
	h m s	h m s	° ′	° ′
entral Eclipse begins at	8 46 8·6 P.M. and	4 35 56·6 A.M.	52 43·7 N.	242 33·0 W.
,, ends	11 15 48·5	6 45 54·5 P.M.	31 17·2	67 28·5
reatest N. Lat. of C. E.	9 18 30	9 40 14·0 A.M.	64 15·7	174 34·0
clipse central at Noon	9 45 59·7	0 5 26·1 noon	61 49·4	145 8·4
uration of Central Eclipse	2 29 39·9.			

Eclipse Central at

Siberia.

G.M.T. h m s	L.M.T. h m	Latitude N. ° ′	Longitude. ° ′
8 46 8·6	4 35·94 A.M.	52 43·7 N.	242 33·0 W.
47 0	5 13·19	55 29·3	233 27·1
48 0	32·25	56 45·2	228 56·3
50 0	59·95	58 24·3	222 30·8
52 0	6 22·31	59 34·6	217 25·4
54 0	41·97	60 29·6	213 0·5
56 0	7 0·04	61 14·3	208 59·4
58 0	16·98	51·5	205 15·3
9 0 0	33·09	62 22·7	201 43·6
2 0	48·54	48·8	198 21·9
4 0	8 3·45	63 10·7	195 8·2
6 0	17·90	28·7	192 1·5
8 0	31·94	43·4	189 0·9
10 0	45·62	55·0	186 5·7
12 0	58·97	64 3·7	183 15·5
14 0	9 11·99	10·0	180 30·1
16 0	24·72	13·9	177 49·2
18 0	37·17	15·6	175 12·5
20 0	49·34	13·1	172 39·9
22 0	10 1·25	12·8	170 11·3

North America.

9 24 0	10 12·89	64 8·7	167 46·6
26 0	24·28	2·8	165 25·8
28 0	35·42	63 55·3	163 8·7
30 0	46·31	46·3	160 55·3
32 0	56·97	35·9	158 45·4
34 0	11 7·41	24·2	156 38·9
36 0	17·61	11·1	154 35·9
38 0	27·59	62 56·9	152 36·1
40 0	37·36	41·6	150 39·6
42 0	46·92	25·2	148 46·2
44 0	56·29 A.M.	7·7	146 55·7
46 0	0 5·46 P.M.	61 49·3	145 8·1
48 0	14·45	30·0	143 23·3
50 0	23·25	9·9	141 41·2
52 0	31·89	60 48·9	140 1·7
54 0	40·36	27·1	138 24·6
56 0	48·68	4·5	136 49·8
58 0	56·85	59 41·2	135 17·3
10 0 0	1 4·88	17·2	133 46·8



G.M.T. h m s	L.M.T. h m	Latitude N. ° ′	Longitude W ° ′
10 2 0	2 12·78	58 52·5	132 18·3
4 0	20·55	27·2	130 51·7
6 0	28·21	1·2	129 26·9
8 0	35·75	57 34·7	128 3·8
10 0	43·18	7·5	126 42·3
12 0	50·52	56 39·7	125 22·2
14 9	57·77	14·4	124 3·4
16 0	2 4·95	55 42·4	122 45·8
18 0	12·06	12·9	121 29·1
20 0	19·11	54 42·9	120 13·4
22 0	26·10	12·3	118 58·5
24 0	33·04	53 41·1	117 44·4
26 0	39·93	9·3	116 31·0
28 0	46·79	52 36·9	115 18·1
30 0	53·63	4·0	114 5·5
32 0	3 0·46	51 30·4	112 53·1
34 0	7·28	50 56·2	111 40·8
36 0	14·12	21·4	110 28·2
38 0	20·98	49 46·0	109 15·3
40 0	27·87	9·7	108 1·9
40 30	29·60	0·4	107 43·5

United States : Territory of Montana.

10 41 0	3 31·33 P.M.	48 51·3	107 25·1
42 0	34·79	32·7	106 48·1
43 0	38·28	13·8	10·8
44 0	41·78	47 54·8	105 33·3
45 0	45·30	35·5	104 55·5
46 0	48·83	16·0	17·5
47 0	52·40	46 56·3	103 39·0
48 0	55·99	36·4	0·1
49 0	3 59·61	16·2	102 20·8
50 0	4 3·27	45 55·7	101 41·0
51 0	6·96	35·0	0·6

Territory of Dakota.

10 52 0	4 10·69	45 13·9	100 19·7
53 0	14·45	44 52·5	99 38·2
54 0	18·27	30·8	98 56·0
55 0	22·13	8·7	13·0
56 0	26·06	43 46·3	97 29·1
57 0	30·05	23·5	96 44·3

State of Iowa.

G.M.T. h m s	L.M.T. h m	Latitude N. ° ′	Longitude W. ° ′
10 58 0	4 34·11 P.M.	43 0·1	95 58·4
59 0	38·25	42 36·3	11·3
11 0 0	42·47	12·0	94 22·9
0 30	44·63	41 59·7	93 58·1
1 0	46·80	47·2	33·0
30	49·01	34·6	7·4
2 0	51·24	21·8	92 41·4
30	53·51	8·8	14·9
3 0	55·81	40 55·6	91 47·9

Illinois.

11 3 30	4 58·13	40 42·3	91 20·5
4 0	5 0·50	28·7	90 52·5
30	2·91	14·9	23·9
5 0	5·35	1·0	89 54·7
30	7·85	39 46·8	24·7
6 0	10·40	32·3	88 54·0
30	13·01	17·5	22·4
7 0	15·67	2·4	87 49·9

Indiana.

11 7 30	5 18·39	38 47·1	87 16·6
8 0	21·19	31·3	86 42·1
30	24·08	15·1	6·3

Kentucky.

11 9 0	5 27·05	37 58·6	85 29·2
30	30·13	41·6	84 50·6
10 0	33·31	24·0	10·4
30	36·61	5·8	83 28·3

S.W. Corner of Virginia.

11 11 0	5 40·06	36 46·9	82 44·1
15	41·84	37·2	21·1

North Carolina.

11 11 30	5 43·68	36 27·3	81 57·3
45	45·56	17·1	32·8
12 0	47·51	6·6	7·4

G.M.T. h m s	L.M.T. h m	Latitude N. ° ′	Longitude W. ° ′
11 12 15	5 49·51	35 55·9	80 41·1
30	51·57	44·9	13·9
45	53·72	33·6	79 45·5
13 0	55·95	21·8	15·8
15	58·27	9·6	78 44·7
30	6 0·70	34 56·9	12·0
45	3·26	43·6	77 37·3
14 0	5·99	29·5	0·2
	Atlantic Ocean.		
11 14 15	6 8·90	34 14·6	76 20·2
30	12·05	33 58·6	75 36·7
45	15·51	41·3	74 48·6
15 0	19·40	21·9	73 54·0
15	23·93	32 59·7	72 49·8
30	29·67	32·3	71 27·5
42	36·39	0·5	69 49·6
45	39·00	31 48·5	11·3
48·0	43·43	28·3	68 5·5
11 15 48·5	6 45·91	31 17·2	67 28·5

The Time of Beginning and End, &c. of the Eclipse of August 7, 1869, at four places where it will be Total.

United States, Boundary.

Lat. 49° 0′ 0″; Long. 107° 42′ 48″.

	Greenwich M.T. h m s	Boundary M.T. h m s
Beginning of the Eclipse	9 31 3·4	2 20 12·2 P.M.
,, Totality	10 38 47·1	3 27 55·9
Apparent Conj. of ⊙ and ☽ in Longitude	10 49 31·6	3 29 40·4
End of Totality	42 15·7	31 24·5
,, the Eclipse	11 44 39·4	4 33 48·2
Duration of Totality	..	3 28·6
,, Eclipse	..	2 13 36·0
At Ap. Conj. Diff. App. Latitudes ☽ − ⊙	..	☽ −0″·15
At Beg. and End of Totality, ☽'s D. − ⊙'s D. = ☽ + 50″·79 and 50″·68		
Point of Beginning, Direct Vision, from ⊙'s Vertex to the right hand		104°

City of Des Moines, Iowa.

Lat. 41° 50′ 24″; Long. 93° 39′ 18″.

	Greenwich M.T.	Des Moines M.T.
	h m s	h m s
Beginning of the Eclipse	9 56 47·0	3 42 9·8 P.M.
,, Totality	10 59 23·0	4 44 45·8
Apparent Conj. of ☉ and ☽ in Longitude	11 0 53·2	46 16·0
End of Totality	2 23·0	47 45·8
,, the Eclipse	59 32·5	5 44 55·3
Duration of Totality	..	3 0·0
,, Eclipse	..	2 2 45·5
At Ap. Conj. Diff. App. Latitudes ☽ − ☉	..	☽ −0″·03
At Beg. and End of Totality, ☽'s D. − ☉'s D. = ☽ + 47″·73 and 47″·60		
Point of Beginning, Direct Vision, from ☉'s Vertex to the right hand		120½°

Springfield, Illinois.

Lat. 39° 49′ 0″; Long. 89° 30′ 0″.

	Greenwich M.T.	Springfield M.T.
	h m s	h m s
Beginning of the Eclipse	10 3 10·4	4 5 10·4 P.M.
,, Totality	11 3 59·7	5 5 59·7
Apparent Conj. of ☉ and ☽ in Longitude	5 25·2	7 25·2
End of Totality	6 50·5	8 50·5
,, the Eclipse	12 2 25·3	6 4 25·3
Duration of Totality	..	2 50·8
,, Eclipse	..	1 59 14·9
At Ap. Conj. Diff. App. Latitudes ☽ − ☉	..	☽ −0″·7
At Beg. and End of Totality, ☽'s D. − ☉'s D. = ☽ + 46″·62 and 46″·49		
Point of Beginning, Direct Vision, from ☉'s Vertex to the right hand		124°

Raleigh, North Carolina.

Lat. 35° 46′ 48″; Long. 78° 38′ 19″.

	Greenwich M.T.	Raleigh M.T.
	h m s	h m s
Beginning of the Eclipse	10 14 46·5	5 0 13·2 P.M.
,, Totality	11 11 20·2	56 46·9
Apparent Conj. of ☉ and ☽ in Longitude	12 22·0	57 48·7
End of Totality	13 23·2	58 49·9
,, the Eclipse	12 5 27·6	6 50 54·3
Duration of Totality	..	2 3·0
,, Eclipse	..	1 50 41·1
At Ap. Conj. Diff. App. Latitudes ☽ − ☉	..	☽ −24″·34
At Beg. and End of Totality, ☽'s D. − ☉'s D. = ☽ + 43″·57 and 43″·46		
Point of Beginning, Direct Vision, from ☉'s Vertex to the right hand		130°



The geographical position of Raleigh is supposed to be well known, but the assumed latitude of Des Moines and Springfield may be too great. A diminution of 10′ therein would make the computed times of the four contacts *later* at the former by 17·2, 19·2, 15·7, and 16·0 seconds, reduce the length of totality 3·5 seconds, and make the Ap. Lat. of the Moon—Sun's at conjunction + 5″·88, and at Springfield later by 16·8, 16·9, 15·4, and 14·0 seconds, reduce the length of totality to 2ᵐ 49ˢ·3, and increase the least distance of centres, Moon North, to 5″·70.

The width of the Moon's shadow in Iowa, Illinois, Indiana, &c., will be about 100 miles.

On the Corona round the Sun in Total Eclipes.

By Jos. Baxendell, Esq.

(*Extract from a Letter to Mr. Huggins.*)

I have been much interested in reading the report of a discussion which took place at the last Meeting of the Royal Astronomical Society, respecting the corona which is seen round the Sun during total eclipses. Recent observations and discoveries indicate very clearly that this corona is not an appendage of the Sun. What then is it? or how is its appearance to be accounted for? In a conversation I had some weeks ago with Professor Roscoe on the subject, I reminded him that in a paper which I read to the Literary and Philosophical Society in March, 1864, giving the results of a discussion of an immense number of magnetical and temperature observations made in different parts of the world, I showed that these results could be best explained by assuming the existence of an irregular nebulous ring circulating about the Sun, nearly in the plane of the ecliptic, and at a mean distance of 0·169; and I suggested to him that the reflexion of the Sun's light from the matter of this ring might be the real cause of the appearance of the corona in total solar eclipses. The view taken by M. Faye and the Astronomer Royal appears to me to be quite inadmissible. During that portion of the time of totality, when the corona is seen to the greatest advantage, no part of the Earth's atmosphere within a considerable angular distance of the Sun and Moon receives any direct sunlight, and, therefore, none can be reflected from it. Nor can I conceive how the corona can be due to any atmosphere about the Moon that could not be detected under other circumstances than those of a total solar eclipse. The differences in the appearances of the corona as seen and described by different observers cannot fairly be adduced as an argument against its cosmical origin; on the contrary, they are precisely what might be expected to occur with such an object seen under slightly different conditions of transparency of the atmosphere, and with eyes of different degrees of susceptibility to faint impressions of light.

Further investigations which I have made since I communi-

cated the paper referred to above to the Literary and Philosophical Society have yielded no results that at all militate against the conclusions at which I had arrived.

Cheetham Hill, Manchester,
May 6, 1869.

On the Determination of the direction of the Meridian with a Russian Diagonal Transit Instrument. By Captain A. R. Clarke, R.E. (Abstract.)

The paper contains an account of the determination of the direction of the meridian at Findlay Seat, a mountain in Elginshire, 1104 feet high, 1868, Oct. 13 to 31, by means of a diagonal transit instrument,—that is, one in which the rays of light (instead of passing straight from the object-glass to the eye-piece) are bent at right angles by a prism in the central cube and so pass out at one of the pivots. The instrument used was made in Russia by M. Brauer, in the workshops of the Imperial Observatory at Pulkowa, and was lent by the Astronomer Royal to the Director of the Ordnance Survey; focal length of telescope about 31 inches, and aperture $2\frac{5}{8}$ inches; distance between the uprights 19 inches. In a copy of the instrument made by Messrs. Troughton and Simms, the telescope has 32 inches focal length, and $2\frac{5}{8}$ inches aperture. The general conclusion is, that considering the inherent difficulty of determining the direction of the meridian in so high a latitude, and the circumstances under which the observations were made,—in stormy weather, on the top of a high mountain, until the party was fairly driven away by snow and the destruction of some of the tents—the results furnish strong evidence in favour of the stability and excellence of the instrument. The observations and calculations were made in connexion with the Ordnance Survey, but were, by the permission of Col. Sir Henry James, presented to the Society. The observations at Findlay Seat were made by Quarter-master Steel and Sergeant Buckle, Royal Engineers.

Account of a series of Drawings of Mars.
By John Joynson, Esq.

The Opposition of *Mars*, which occurred on the 13th February last, has not been a favourable one for observations of the appearances on his disk; and, in addition to this, observers have had to contend with most unpropitious weather, the moist, open winter rendering our own atmosphere almost entirely unfit for the noting of the markings presented when the planet could be seen.

The accompanying drawings are selected from ninety, which

I have been able to make at various times. They represent the planet during a complete rotation, and are intended as a sequel to those sent to the Society for the three previous Oppositions. They are so arranged that each diagram on the several sheets represents the same aspect of the planet, the appearance being, of course, altered somewhat at each opposition by the change in apparent position of the axis of the planet.

I have endeavoured, through the Society, to interest others in obtaining similar sets; but so far I am sorry to say that I have not met with much apparent success. It will be readily seen that there has been no absolute change in the Band, and the Channel from it to the North Pole, since the first set of diagrams were made in 1862. It is true that the set now sent appears to differ from that first taken, and it would be difficult at first sight to say that they were views of the same aspect of the planet; but those for 1864 and 1867 will sufficiently prove that to be the case, as the gradual alteration can by their aid be clearly seen. I have no doubt but this will continue during the future oppositions, and that it will be found that the same appearances will return in due course.

It has been observed at each opposition that the power of the eye-piece used, and also the presence or absence of hazy cloud, materially affected the colour of the disk; and I am disposed to think that the reddish look the planet sometimes had was chiefly, if not entirely, caused by our own atmosphere, as at these times even the reddest parts looked brownish, and sometimes yellow.

The colour of the Band and Channel has been dark, without any of the greenish appearance noted in former oppositions.

Waterloo, near Liverpool,
April 7, 1869.

Remarks on Mr. Joynson's Paper of Occultations.

By John J. Plummer, Esq., Observer at the Durham Observatory.

In the *Monthly Notices*, vol. xxix. No. 5, are some observations of Occultations by Mr. Joynson, observed near Liverpool, accompanied by some remarks, the object of which seems to be, to express a doubt as to the accuracy of the approximate times of these phenomena, given in the *Nautical Almanac*. Believing that the error was most probably on the part of Mr. Joynson, I computed for the three stars, *Aldebaran*, 119 *Tauri*, and 120 *Tauri*, the times of their disappearance and reappearance at Greenwich, with sufficient approximation; and it is perhaps superfluous to add, with results absolutely identical with those of the *Nautical Almanac*. I also computed by the same formulæ the times of the occultations of these stars for the latitude and longitude of Mr. Joynson's Observatory. The following are the results:—

1869.			Calc. G.M.T. for Greenwich.	Angle from North Point.	Calc. G.M.T. for Liverpool.	Angle from North Point.	Obs^{d} G.M.T. at Liverpool.
			h m	o	h m	o	h m s
Jan. 23	Aldebaran	Dis.	8 48·7	97	8 44·6	105	8 44 16
		Reap.	10 4·7	289	10 0·0	282	10 0 0
24	119 Tauri	Dis.	8 14·5	143	8 18·0	155	8 17 26
		Reap.	9 9·5	237	9 0·2	225	9 0 45
24	120 Tauri	Dis.	8 47·8	117	8 46·7	126	8 46 27
		Reap.	9 59·6	260	9 53·1	252	9 48 13

It will be seen that the agreement between the observed and computed times for Liverpool is sufficiently exact, with the exception of that for the last star, the observed time for the reappearance of which appears to be 5 minutes in error. The effect of the difference of parallax upon the observed time of the occultation of a star by the Moon, evidently, cannot be obtained without systematic calculation.

Permit me to call the attention of those astronomers who observe occultations at places remote from Greenwich, to a paper by the Rev. T. Chevallier, published in vol. xix. of the *Memoirs* of the Royal Astronomical Society, by which the effect of parallax on an occultation is readily calculable. I have observed during one year above twenty occultations, the approximate times of which have always been computed beforehand by tables founded upon that method. This computation is the work of less than two minutes, and the mean error in the approximate time thus obtained, I have found to be 34 seconds, a sufficient degree of accuracy, since the times in the *Nautical Almanac*, and upon which the computation depends, are given only to the nearest minute. What is perhaps of more importance, especially for observations of reappearances at the bright limb, is, that the approximate angle from the north point of the Moon is obtained with a mean error of $\frac{7}{10}$ of a degree only. Those who have computed the corrections due for the effect of parallax at different places by the long and rigorous formula, will appreciate the value of this short cut; and I shall be happy to compute tables upon the same plan as those I have myself used, for any Observatory where their value may be appreciated.

Durham Observatory, April 16th, 1869.

On the Period of η Argûs. By John Tebbutt, Jun.

(*Extract from a Letter to Sir J. F. W. Herschel, Bart.*)

"I have read with much interest the paper by Mr. Abbott on η *Argûs* and its surrounding nebula, at page 200 of the 28th

vol. of the R. A. S. *Monthly Notices*, and your own remarks at page 225 of the same volume. You are already, doubtless, aware that I have carefully watched the variations in the light of this star during the past fifteen years. As it is not improbable that the star may soon begin to increase in lustre, I have watched it for the past twelve months, and am still watching it, with more than ordinary interest, in order to secure, if possible, the time and magnitude of the minimum. I have, therefore, on several occasions since April last carefully compared η *Argûs* with the principal stars in the Catalogue on page 42 of your 'Results of Astronomical Observations at the Cape of Good Hope from 1834–8.' In order to be assured of the identity of the comparison stars with the principal ones in your Catalogue, I determined their positions approximately by means of a telescope of 48 inches focal length and 3¼ inches aperture, roughly mounted as an equatoreal, a transit telescope of 2·1 inches aperture mounted on Y's secured to substantial piers of brick and stone, and a half-seconds chronometer. The comparisons with the telescope of 3¼ inches aperture were made with a ring-micrometer, whose diameter = 21′ 3″·6. The following Table gives the individual results of the comparisons, η *Argûs* being in each case employed as the comparison star:—

"*Co-ordinates with reference to η Argûs of certain Stars (Mags. 6½ to 7) in the Catalogue on page 42 of Sir J. Herschel's 'Results of Astronomical Observations at the Cape of Good Hope*, 1834–8.'

Date of Comps.	No. 1 or L. R.A.	No. 1 or L. Decl.	No. 2 or X. R.A.	No. 2 or X. Decl.	No. 522 or U. R.A.	No. 522 or U. Decl.
1868.	m s	′ ″	m s	′ ″	m s	′ ″
May 19	...	...	...	...	0 37·85 W.	18 24·9 S.
21	3 45·36 W.	17 29·9 S.	3 41·35 W.	0 12·3 N.	0 37·98 W.	18 30·4 S.
	...	...	...	...	0 38·86 W.	18 37·3 S.
	...	...	...	...	0 39·11 W.	18 35·6 S.
	...	...	...	...	0 38·61 W.	18 27·3 S.
22	3 45·62 W.	17 29·6 S.	3 42·35 W.	0 13·7 N.	...	...
Means	3 45·49 W.	17 29·8 S.	3 41·85 W.	0 13·0 N.	0 38·48 W.	18 31·1 S.

Date of Comps.	No. 1183 or W. R.A.	No. 1183 or W. Decl.	No. 1203 or Y. R.A.	No. 1203 or Y. Decl.
1868.	m s	′ ″	m s	′ ″
May 19	2 38·31 E.	...	2 58·99 E.	13 43·0 S.
21	2 38·38 E.	11 17·4 S.	2 58·30 E.	13 53·2 S.
	2 37·81 E.	11 10·5 S.	2 59·11 E.	13 50·1 S.
22	...	...	2 59·12 E.	...
Means	2 38·17 E.	11 14·0 S.	2 58·88 E.	13 48·8 S.

The letters N. S. E. and W. denote whether the star is north, south, east, or west of η *Argûs*.

"By two comparisons of No. 387 with 1183, on the 22d May, I found that the former was $3^m\,45^s{\cdot}50$ *west* and $14'\,44''{\cdot}5$ *south* of the latter. Adopting $2^m\,38^s{\cdot}17$ E. and $11'\,14''{\cdot}0$ S. as the position of No. 1183, with reference to *η Argûs*, we have for the position of No. 387 or Z, with reference to *η Argûs*, as follows:—

R.A. = $1^m\,7^s{\cdot}33$ *west*. Decl. = $25'\,58''{\cdot}5$ *south*.

"I have also observed the stars with the transit instrument, but these observations give only the difference of R.A. The co-ordinates of the stars as I have given them agree with your Cape Results, precession &c. considered, within quantities which may be regarded as due to errors of observation rather than to changes in the relative positions of the stars.

"I trust the foregoing results, imperfect as they are, may be of interest to you, and I hardly need say you are at liberty to make what use of them you may think proper. Not being a Member of the Society, I do not receive the *Monthly Notices* as they are issued: hence the lateness of this communication.

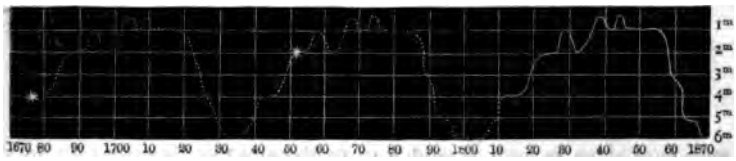
"*Windsor, New South Wales,*
February 9th, 1869."

On the Period of η Argûs. By Prof. Elias Loomis.

In the *Astronomische Nachrichten*, vol. lx. p. 61, Prof. Wolf has endeavoured to show that the observations of *η Argûs* may be represented by a period of forty-six years, with one principal maximum and two secondary maxima in the intensity of its light. The observations recently published by Tebbutt (*Monthly Notices*, vol. xxviii. p. 266) indicate that this period is too short. I find that all the observations of the present century may be represented by supposing the period to be sixty-seven years; and if the curve representing the variations of brightness be twice repeated it will perfectly represent the two observations of 1677 and 1751. Future observations may require us to modify this conclusion; but at present it seems probable that *η Argûs* is a periodic star whose changes of brightness vary from the first to the sixth magnitude, and whose period is about seventy years.

The following table contains all the observations of this star with which I am acquainted, and the accompanying figure shows the curve which represents them.

Date.	Mag.	Authority.	Date.	Mag.	Authority.
1677	4	Halley	1845	1	Jacob
1751	2	Lacaille	1850	1	Gilliss
1811	4	Burchell	1854	1	Tebbutt
1815	4	Burchell	1856	1·5	Powell
1822	2	Fallows	1859	3·1	Powell
1826	2	Brisbane	1861	3·6	Powell
1827	1	Burchell	1862	4·9	Tebbutt
1832	2	Johnson	1863	5·1	Tebbutt
1835	1·4	Herschel	1865	5·2	Tebbutt
1838	0·5	Herschel	1866	5·8	Tebbutt
1842	1	Maclear	1867	6	Tebbutt
1843	0·5	Maclear	1868	6	Tebbutt



If you consider this notice of sufficient importance you are at liberty to communicate it to the Astronomical Society.

Yale College, April 9, 1869.

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*Extract of a Letter from Dr. A Winnecke to E. J. Stone, Esq.**

"I have to announce to you (for communication to the Royal Astronomical Society) the discovery of a Comet, which I observed yesterday night:—

	h m			h m s
April 9	12 24	Karlsruhe M.T.	α Comet	10 32 11
			δ Comet	+ 33° 57′

"This stranger will prove doubtless the expected Comet 1858 II., in the track of which it is situated. If I suppose the identity of the two bodies, the perihelion passage will take place June 25 or 26. It is faint, but large, 6′ perhaps 8′ in diameter.

"*Karlsruhe*, 1869, *April* 10."

* Previously issued as a circular, but inadvertently omitted from the last number of the *Monthly Notices*. Ed.

Observations of Winnecke's Comet. By Hall Wortham, Esq.

1869.	Comet's R.A. h m s	Comet's N.D. ° ′ ″	
April 29	10 3 0	36 32 0	(*a*)
May 4	9 57 0	36 42 0	(*b*)

Telescope employed, an achromatic of 6 inches aperture; power 50.
The above positions are *approximate* only.

(*a*). Comet distinctly seen as a faint nebulous patch of some little size, appearing occasionally to brighten somewhat to a centre, and followed at about its own diameter by a small, but brightish, star below the 9th magnitude. A larger star just beyond the upper part of the field. Telescope set by the Ephemeris by Dr. Winnecke (appearing in No. 1753 of *Astron. Nach.* p. 14) and comet found at once nearly in centre of field of view.

(*b*). Night not very clear, and the comet only just visible.

Royston, Herts, May 4th, 1869.

Observations of the same Comet. By S. J. Perry.

The nights following the 11th and 12th of this month were the only ones clear enough to enable us to observe the Comet which is approaching us at present. On May 11th, $14^h\ 7^m\ 13^s{\cdot}7$ Sid. T., the position was

R.A. $9^h\ 49^m\ 32^s{\cdot}7$ Decl. + 37° 4′ 10″

May 12th, $14^h\ 12^m\ 47^s{\cdot}16$,

R.A. $9^h\ 48^m\ 37^s{\cdot}16$ Decl. + 37° 3′ 45″.

These observations are uncorrected for refraction. If it would be of any use I could give you the positions for future fine nights, but I suppose there are so many good astronomers watching it at present that our observations will be of little use. There seems to be a slight condensation towards the centre, but no decided nucleus. This is all that our object-glass will give. I used a power of about 100, with full aperture 8 in.

I have just laid down a telegraphic wire between our two Observatories, which enables me to give the time and R.A. very correctly.

Stonyhurst College, Blackburn,
May 13, 1869.

Note on a Sun-spot seen March 14, 1869.
By John Browning, Esq.

On the above date I was observing the Sun about 9.30 A.M.

Saw a large Sun-spot about 20° N. of the equator, and about 2′ from the E. limb.

Definition being very good I was able to use 12 inches aperture and achromatic power 148 and 208.

To modify the light, I employed a *double*-prism solar eyepiece, and a corrected wedge of neutral tint glass.

The dimensions of the spot I found to be from N. to S. 14,400 miles, from W. to E. 19,600 miles. When comparing these measures with the drawing, it must be recollected that due allowance has been made in this calculation for the great foreshortening which occurs, owing to the spot being so near the edge of the limb.

The umbra contained three nuclei of very unequal dimensions, arranged nearly in the form of an equilateral triangle.

The drawing I made of the spot I have the honour to submit to the Society.*

Two bridges crossed the spot at an angle of about 40°. These bridges presented the appearance of broken twigs lying mostly in the direction of the bridges. It will be seen that both the bridges appear to be fed from a point to the N.E. of the spot.

The edge of the penumbræ furthest from the Sun's limb consisted almost entirely of bright faculæ. The extreme edge of the penumbræ was much darker than other portions, a point lying S.E. being the darkest of all.

This distinct darkening of the extreme edge of the penumbræ has often been doubted and ascribed to the effect of contrast; but if the spot were spirally formed, it would, I think, present some such appearance as I have described.

I have heard from Mr. G. P. Bidder, Jun., that on the afternoon of May 13, he saw a fine large spot, in which the bridges were spirally arranged. This observation I think very interesting.

During moments of the best definition the photosphere resembled broken branches, covered with long leaves, and overlying each other in all directions.

I would wish to speak with all diffidence on such a difficult matter of observation, but I cannot help thinking that the smaller granulations on the photosphere change considerably in appearance. I have seen them in the form of broken twigs, rice grains, and willow leaves, that is, sometimes blunt and sometimes sharp at the extremities, while at other times they were little more elongated than a common egg.

The very careful and interesting series of drawings taken by the Rev. F. Howlett show that Sun-spots have a tendency to assume regular forms. The cloudy matter which forms the photosphere, may frequently assume an appearance similar to that we may occasionally see in our own clouds, sometimes spoken of as "mackerel-back" clouds. If this be so it will be of little use to dispute as to whether these bodies are shaped like rice-grains or willow-leaves. We had better simply note their forms whenever we are so fortunate as to observe them distinctly.

* The drawing was exhibited at the Meeting. Ed.

On a Remarkable Sun-spot observed May 1st, 1869.
By G. P. Bidder, Esq.

The accompanying drawing* represents the largest of a very fine group of spots, as it appeared upon the afternoon of Saturday, May 1st. It was chiefly remarkable for a distinctly apparent spiral arrangement, which was very striking. The whole of it gave the impression of an immense whorl, as shown in the drawing, which by no means exaggerates the resemblance.

The spot was traversed by an unusually attenuated bridge, which also appeared to participate in the spiral arrangement. The shortness of time during which the Sun was visible prevented my doing more than sketch the outline of the spot. One measurement was obtained which showed the penumbra to be in length about 27,000 miles. I examined the same spot again on the following day, when all traces of a spiral arrangement had entirely disappeared.

On a proposed Form for Spirit Levels. By Major Tennant.

My late employment has caused my thoughts to revert to some old projects for improving the spirit-level, and the subject is of much importance now that reliance is being daily more and more placed on the indications of these accessories. My original examination was, I think, in 1853, when I entered into an investigation of the properties a spirit-level should have on the supposition that the motion of the bubble was made by bodily transfer.

This hypothesis is widely removed from fact if we look to great movements of the level, but very nearly true for small ones,† and it leads me to a form of level and mode of construction which I have never seen elsewhere, and which I still think would produce results far superior to and far more certain than those of the present process.

I arrived at the conclusion which will, I think be received, that *the volume or contents of a bubble should be large, that its density relatively to that of the fluid should be small, and that its floor, or the surface common to the fluid and bubble, should have as large an area as possible,* and that these properties would be best gained by increasing the radius of curvature of the upper

* The drawing referred to was exhibited at the Meeting.

† If we make an alt-azimuth revolve about its vertical axis smartly, we see the bubble pass to the end of the tube, then to the other, and finally settle; and any great and sudden change of level causes a wave to pass along the floor of the bubble till equilibrium is established: but if only a small change be made by the foot-screw the bubble moves sensibly as one mass.

art of the level tube and making the fluid as heavy as possible compared with the bubble. In order that temperature should produce as little effect as possible on the length of the bubble, I would also reduce the volume of fluid, and to gain all these points I proposed to abandon altogether the cylindrical form of level tube and to cut out the necessary cavity from a rectangular prism of glass by means which should ensure that the longitudinal section of the cavity should be a segment of a circle and the transverse one uniform.

I proposed to take a prism, A B C D, of glass and to grind out of it a cavity of a somewhat elliptical shape transversely, as shown in the figure on the margin and in longitudinal section as below. C D E F is a flat plate cemented on to close the level.





Instead of filling the cavity with a volatile fluid of small density, whose vapour mixed with air forms the bubble, I think that the strength this new form might have would allow the use of mercury for the fluid and hydrogen perhaps (it would be necessary to exclude air) for the bubble. If mercury could be used it might be possible to float on it a plate of glass or iron carrying a mark, whose position could be referred to a scale with far more accuracy than the ends of the bubble can now be read; but I fear that this would be quite impracticable, and that the plate would have to be rejected from its liability to stick, if it fitted to the shape of the bubble floor closely enough to be useful.

At the time I have mentioned, I proposed, in a letter to the Surveyor General, to try making a level of this sort but difficulties intervened of various sorts, and it was laid aside. Any one probably would be able to devise the necessary apparatus for cutting the glass truly, but what I proposed was to have a cutting wheel revolving rapidly about a vertical axis and to traverse the glass plate before it. If it were rigidly fixed to two straight edges guided by cylindrical studs on a table, the longitudinal section would be a circle, and the form of the final cutting wheel would define the cross section.

It is hardly possible that I shall now be able to try this project (which I had nearly forgotten), but I have mentioned it to several persons, and I now offer it to the Astronomical Society in the hope that it may lead to an improvement in the mode of constructing levels.

Calcutta, September 25th, 1868.

ERRATUM.

In Vol. XIX. of the Memoirs of the Royal Astronomical Society.

Page 234, line 13 from the top,—

for $B = \varrho_2 \cos \phi_2 - \varrho_1 \cos \phi_1$, *read* $B = \varrho_2 \sin \phi_2 - \varrho_1 \sin \phi_1$.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *June* 11, 1869. No. 8.

Admiral MANNERS, President, in the Chair.

W. W. Magee, Esq., University College, London,

was balloted for and duly elected a Fellow of the Society.

Note on the Transit of Venus in 1874.

By J. R. Hind, Esq., F.R.S.

In the *Comptes Rendus* for 1861, July 22, will be found the results of a careful calculation by myself of the circumstances of the transits of 1874 and 1882 derived from M. Le Verrier's Tables of the Sun and Planet. Of the accuracy of the Right Ascensions and Declinations which I employed there can remain no doubt whatever; they have not only been confirmed by a new computation conducted in the *Nautical Almanac* Office, but the Astronomer Royal has since obtained identical results. I was, therefore, somewhat surprised to find that the times of contacts of the centre of *Venus* with the Sun's limb, given by M. Puiseux in the Appendix to the *Connaissance des Temps* for 1871, differed sensibly from the time I assigned in the *Comptes Rendus*, the same tables having been employed in both calculations. I had not much faith in the efficacy of a re-examination by myself of my own calculations, but Mr. Bishop having allowed me to place the matter in the hands of his assistant, Mr. W. Plummer, a very able and accurate computer, I am enabled to state that my published numbers are nearly correct, and that errors must have

crept into M. Puiseux's work. The times deduced by Mr. Plummer are—

	G.M.T. h m s	Difference Plummer — Hind. s
Ingress.		
External contact, December 8th	13 46 42	— 14
Internal contact, ,,	14 15 30	— 27
Egress.		
Internal contact, December 8th	17 57 8	+ 3
External contact, ,,	18 26 6	+ 1

These differences for such a phenomenon are insignificant. The possible errors of any predictions of the times of contact must be very much larger.

1869, *June* 10.

The Transit of Venus in 1874. By R. A. Proctor, B.A.

In the *Monthly Notices* for March last, I indicated my intention of applying more exact modes of examination to the circumstances of the approaching transits of *Venus*, than had hitherto been considered necessary. In resuming the examination of the transit of 1874, I feel it necessary to call attention to that statement of my object, lest it should be thought that the corrections which I have been led to make on former results, point to errors in the processes by which those results were obtained, whereas, in reality, they are only due to the intentional neglect in prior work of considerations which even now many may be disposed to look upon as unnecessarily exact for the purposes of a preliminary inquiry. To still further obviate this objection (which, however, is not likely, I think, to occur to any one who has read my former papers), I shall for the most part refer to former results as those which have been obtained on such and such suppositions. This is the more advisable, because I find that astronomers are willing to accord the same degree of accuracy to my results in relation to the modes by which they have been obtained, as I am ready to assign to others results with a corresponding *proviso*.

It may be necessary, however, before proceeding to a detailed examination of the circumstances of the coming transit, to inquire (1) how far it is necessary or advisable to aim at exactness in the preliminary investigation of the subject; and (2) whether the phase which I have selected for examination is well chosen.

As a reason for applying a comparatively rough mode of

examination to the circumstances of the transit, it has been urged that there is some uncertainty as to the exact moment of conjunction of *Venus* and the Sun, and as to the exact distance which will separate their centres. What is the range of possible error due to this cause I will leave to others to determine. My own impression is, that it is very small, whether considered absolutely or with reference to other sources of error which it is in our power to remove. But whether this be so or not, it seems clear to me that the possible errors, as to time and position-angle, arising from this cause, must be looked upon as affording additional reason for exactness in other matters. For, should any error resulting from disregard of exact considerations happen to be *additive* to errors resulting from inaccuracy in the Planetary Tables, it might happen that the conclusions to which we should be drawn would affect appreciably the success of observations to be made upon the transit.

The possible inaccuracy of the Planetary Tables seems to be the only source of error which it is out of our power to get rid of altogether. Therefore, as the choice of stations depends in certain cases on considerations of some nicety, it seems to me that the only basis for a proper selection is the determination, with as much accuracy as is readily attainable, of the circumstances under which the phase selected for observation will be seen at different stations. It must be remarked, however, that the formulæ and calculations by which I have determined these circumstances are not by any means necessary. A process which I will presently indicate suffices to give, with scarcely any labour, all the necessary information respecting a large number of stations, and that with a degree of accuracy amply sufficient for all the purposes of the inquiry. The only way, however, in which I could conveniently present my results for the study of those who take interest in the subject of this transit was that which I have selected.

As to the selection of internal contacts as the phase to be specially dealt with, there may be, of course, a variety of opinion. I am told, for instance, that in France there is some question of observing external contacts also. In this case, it would be advisable to make external contacts the determining phase (if a choice had to be made), or to determine for several stations the circumstances of both phases. For as at ingress external contact takes place about half an hour before internal contact (for every station), it is clear that the stations for observing retarded ingress (always so selected that the Sun is lately risen) must be chosen with reference to external contacts, if it is a *sine quâ non* that these are to be observed. If such a station were selected with reference to internal contact, with a moderately good solar elevation (necessarily *soon after* Sunrise), it is obvious that at the epoch of external contact half an hour earlier, the Sun would be too low; and considerations of the same sort apply to the observations of accelerated egress.

It appears to me for these reasons that the consideration of both phases is inadmissible, and that though external contacts may be observed with advantage when they happen to be visible, *as well as* internal contacts, yet the stations must be chosen with strict reference to the latter phase, which is admittedly far the better of the two.

As M. Puiseux has given reasons for selecting the passage of *Venus' centre* over the Sun's limb as the phase to be considered in the preliminary inquiry, it may be well to examine how far those reasons are valid. He remarks that what is actually observed is the exterior or interior contact, but that, as the epochs of these phases are separated by nearly constant intervals for all stations, we may neglect the consideration of those intervals when all that we require is to determine the relative values of different stations. I venture to submit the following reasons for looking upon this line of argument as inadmissible:—

It is to be noted, in the first place, that the interval between external and internal contact is not constant, and although its range (from $26^m\ 24^s$ to $31^m\ 18^s$) may seem small, yet, where the values of stations run close, the differences thus resulting are quite sufficient to turn the scale in several instances. But also we may look at the matter in this light: the change of phase results in a change in the position of the stations of maximum acceleration, or retardation, and this change amounts in certain cases to hundreds of miles. Now the circles which give a definite coefficient of parallax in one of these cases, have equal radii, but their poles (the stations just mentioned) are separated by a considerable distance. Therefore the circles must intersect. At a point of intersection the coefficients are equal for either phase: on one side of that point, the nominal value of the coefficient is improved by the change of phase; on the other, it is deteriorated. This proves that the relative values of stations cannot remain unaffected by a change of phase, and suffices to account for the corrections introduced by my consideration of internal contacts in place of the passages of *Venus'* centre.

It would be incorrect, however, not to mention that the larger part of the corrections I now propose to consider, results from another cause than the change of phase; namely, from the effects of parallax,—which it has not hitherto been thought necessary to take into account. The application of the correction due to the equation of time has also to be mentioned as appreciably affecting the extent of the changes which are here dealt with.

I may summarise these changes under three heads:—

1. *The application of Delisle's method of absolute time differences.* The relative as well as the absolute values of many stations are affected by the change of phase. Some which had hitherto appeared unsuitable are found to be unobjectionable. Others which seemed good appear unfit. In other cases the relative values of two stations are so affected that the results of a comparison between them are directly reversed. Lastly, many

stations not hitherto thought of in connexion with the transit are found to be well suited for the application of Delisle's method.

2. *The comparison between Delisle's and Halley's methods.* Halley's method is found, not merely to be applicable with advantage, which is all that can be said of it when central passages are considered, but to be superior to Delisle's,— slightly, when reference is made only to such stations as had been hitherto dealt with, noticeably when Antarctic stations are made use of.

3. *The comparison between the transits of* 1874 *and* 1882 *with reference to Halley's method.* This comparison, conducted according to the principles laid down by Mr. Stone (than whom no one is better entitled to pronounce authoritatively on such points) shows that Halley's mode may be applied much more advantageously to the transit of 1874 than to that of 1882.

I premise that I have carefully gone over all the calculations on which the results of my former paper were founded, and have applied so many independent modes of calculation that I can no longer entertain any doubt as to the accuracy of those results.*

The results to be now brought into comparison for the sake of forming an estimate of the effect of phase, parallax, and the equation of time, upon the values of various stations, may be thus classed:—

A, those derived from the consideration of central passages, as supposed to be seen from the Earth's centre, with the position-angles corresponding to external contact.

B, those derived from the same phase, similarly seen with the position-angles corresponding to central passage.

C, those derived from the consideration of internal contacts, as seen from the stations themselves, and with the position-angles corresponding to the phase so seen.

Note.—The results under head C have alone been corrected for equation of time.

The following table exhibits the position-angles and epochs (for ingress) corresponding to A, B, and C:—

	Position-Angles.			Epochs.		
	A	B	C	A	B	C
	°	° ′	° ′	h m	h m	h m
Accelerated Ingress	131	133 29	133 56	14 0	13 55	14 4
Mean Ingress	131	133 29	136 28	14 0	13 55	14 16
Retarded Ingress	131	133 29	139 29	14 0	13 55	14 29

In preparing the columns under head C in the accompanying

* This paper had originally exhibited an independent mode of establishing my conclusions, founded on a simple construction applied to the elements given in Mr. De La Rue's paper on the transit (*Monthly Notices* for December 1868). For the sake of brevity, I have thought it best to omit this part of the paper, the accuracy of my conclusions not being, I find, called in question. The mode of construction is, however, briefly referred to further on.

tables, I have made use of six orthographic maps which accompany this paper. These were constructed with every precaution to insure accuracy. The intersection of longitude-lines and latitude-parallels (to every 10°) were separately constructed for by a double process, and in all critical cases further tests were applied. In all, the construction of the maps involved upwards of 3000 measurements, and from the scale on which the maps were constructed, the practice I have had in orthographic mapping, and the care used in the present instance to insure accuracy, I feel that I may safely claim for the numbers under the columns headed C in the following tables, a satisfactory degree of trustworthiness.

The six maps include four quarter-spheres, exhibiting the solar elevations and the coefficients of parallax in the same way as in the maps 1-4, in the *Monthly Notices* for December last, though in a different projection. The other two exhibit the Earth as supposed to be seen from the Sun at ingress and egress (*mean*, and for internal contacts). In these the solar elevations are indicated by circles, and in place of 10 parallactic parallels, corresponding to the parallactic circles in the other map, there are laid down parallactic lines corresponding to minute intervals (the line across the earth's centre being taken as a zero-line). These lines are not parallel, but separately constructed for. Thus their indications differ somewhat from those derived from the parallactic circles in the other maps, which are laid down on the supposition (not strictly correct) that the outline of the penumbra of *Venus* travels parallel to itself across the face of the earth. This will account for a slight want of correspondence between the second and third C-columns in the following tables: the third gives the correct effect of parallax. It will be noticed, however, that the difference is always trifling in the case of places suitable for the application of Delisle's method.

A further correction, but one of small importance, would result from the consideration that the apparent outline (supposed to be seen from the Sun) of that part of *Venus*' penumbra which traverses the Earth is not a straight line, but part of a large circle. Thus the actual outline on the Earth's surface is not part of a circle. It follows that the parallactic curves in the four quarter spheres ought not to be circular, and the parallactic lines across the other two maps ought to be curved, the direction of their curvature being turned in the direction towards which the shadow is moving for ingress, and the reverse for egress. All the corrections due to this cause are minute, and attain their greatest values at places not suitable as stations either for the application of Delisle's or Halley's method.

A correction has been applied to columns C corresponding to the fact that the maps are severally constructed for a single epoch, while the events to which they relate occupy several minutes.



Table I. *Accelerated Ingress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Acceleration in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
Woahoo	22·5	..	19·8	·92	·93	..	11·2
Hawaii	22·3	21·5	19·7	·92	·92	10·3	11·1
Aiton I. (Aleutian)	12·0	..	10·8	·80	·84	..	10·3
Marquesas I.	20·0	23·1	17·7	·71	·66	7·5	7·9
Mouth of Amoor R.	15·0	..	14·0	·57	·62	..	7·6
Jeddo	..	30·9	32·1	..	·50	..	6·8
Otaheite	34·5	34·3	29·7	·59	·54	6·1	6·4
Nertchinsk	..	..	10·1	..	·41	..	5·8
Tsitsikar	..	..	17·0	..	·43	..	5·8
Kirin Oula	..	..	19·5	..	·42	..	5·7
Nagasaki	..	..	32·7	..	·40	..	5·3
Tientsin	..	..	22·2	..	·38	..	5·0
Pekin	..	20·2	20·8	..	·30	3·0	4·3
Shanghai	..	29·5	28·5	..	·25	2·6	3·9
Nankin	..	..	27·1	..	·20	..	3·6
Canton	..	..	35·5	..	·08	..	1·6
Hongkong	..	..	36·2	..	·08	..	1·6

Table II. *Retarded Ingress.*

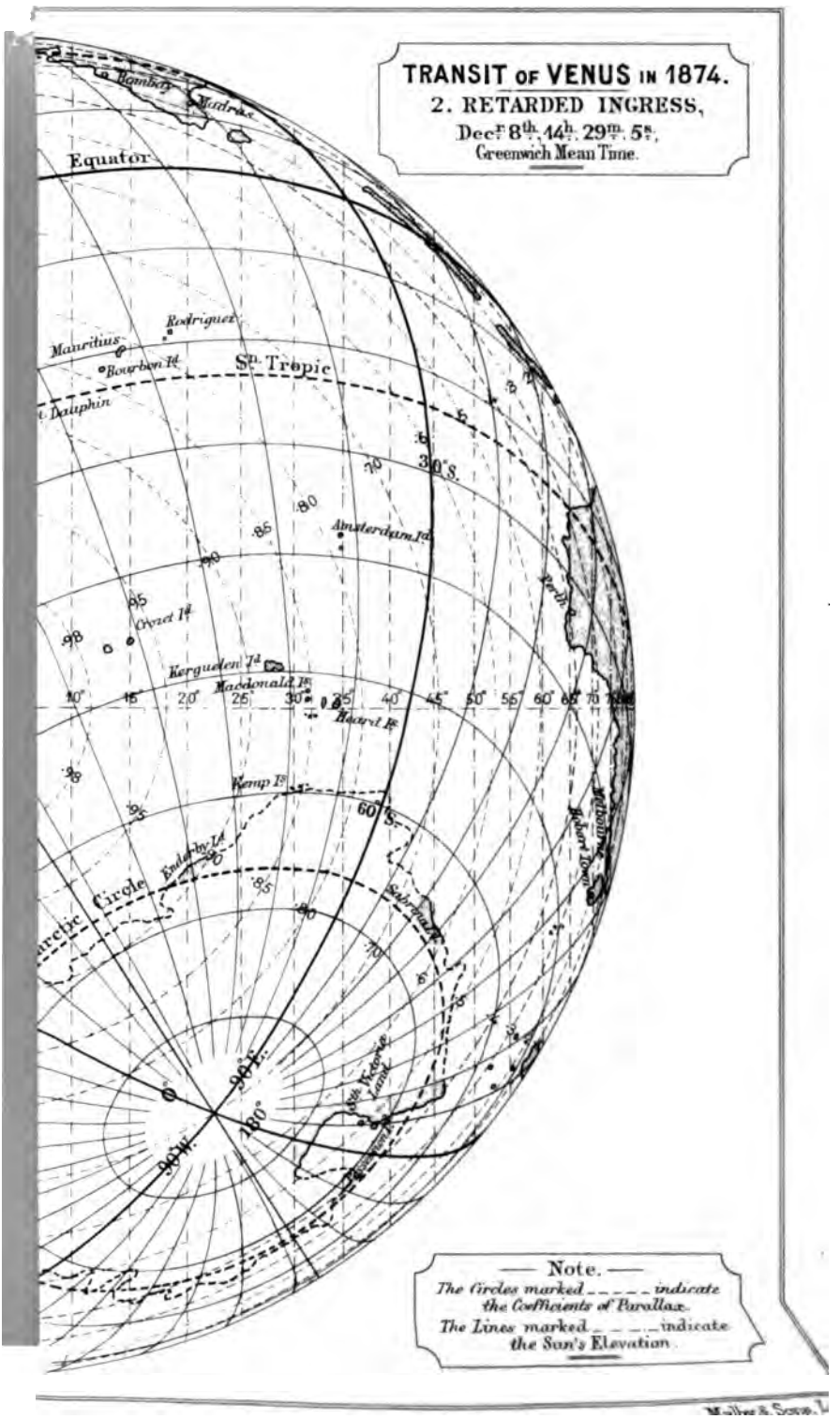
Station.	Sun's Elevation.			Coefficient of Parallax.		Acceleration in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
Crozet I.	9·5	..	15·0	·98	·96	..	12·6
Enderby Ld.	..	17·3	20·0	..	·92	10·3	11·8
Kerguelen Ld.	25·0	23·6	27·5	·91	·88	10·3	11·6
Macdonald I.	..	27·2	31·0	..	·85	..	11·2
Kemp I.	..	..	30·0	..	·85	..	11·1
Bourbon I.	4·5	..	12·4	·93	·84	..	11·1
Mauritius	6·0	..	14·1	·92	·81	..	10·7
Amsterdam I.	..	27·6	34·1	..	·77	9·8	10·3
Rodriguez	11·5	..	19·0	·89	·76	..	9·9
Sabrina Ld.	..	..	45·0	..	·73	..	8·2
Adelie Ld.	..	..	45·0	..	·50	..	6·8
S. Victoria Ld.	..	36·4	38·5	..	·46	5·0	6·0
Perth (Aust.)	..	..	65·0	..	·38	..	5·3
Royal Co. I.	..	..	62·0	..	·32	..	4·5
Madras	12·5	..	21·0	·47	·25	..	4·0
Bombay	4·5	..	12·5	·44	·22	..	3·8
Macquarie Ld.	..	..	52·0	..	·25	2·9	3·5
Hobart Town	..	70·0	67·0	..	·20	..	2·8
Adelaide	..	..	75·0	..	·18	..	2·5
Melbourne	..	..	75·0	..	·13	..	2·2

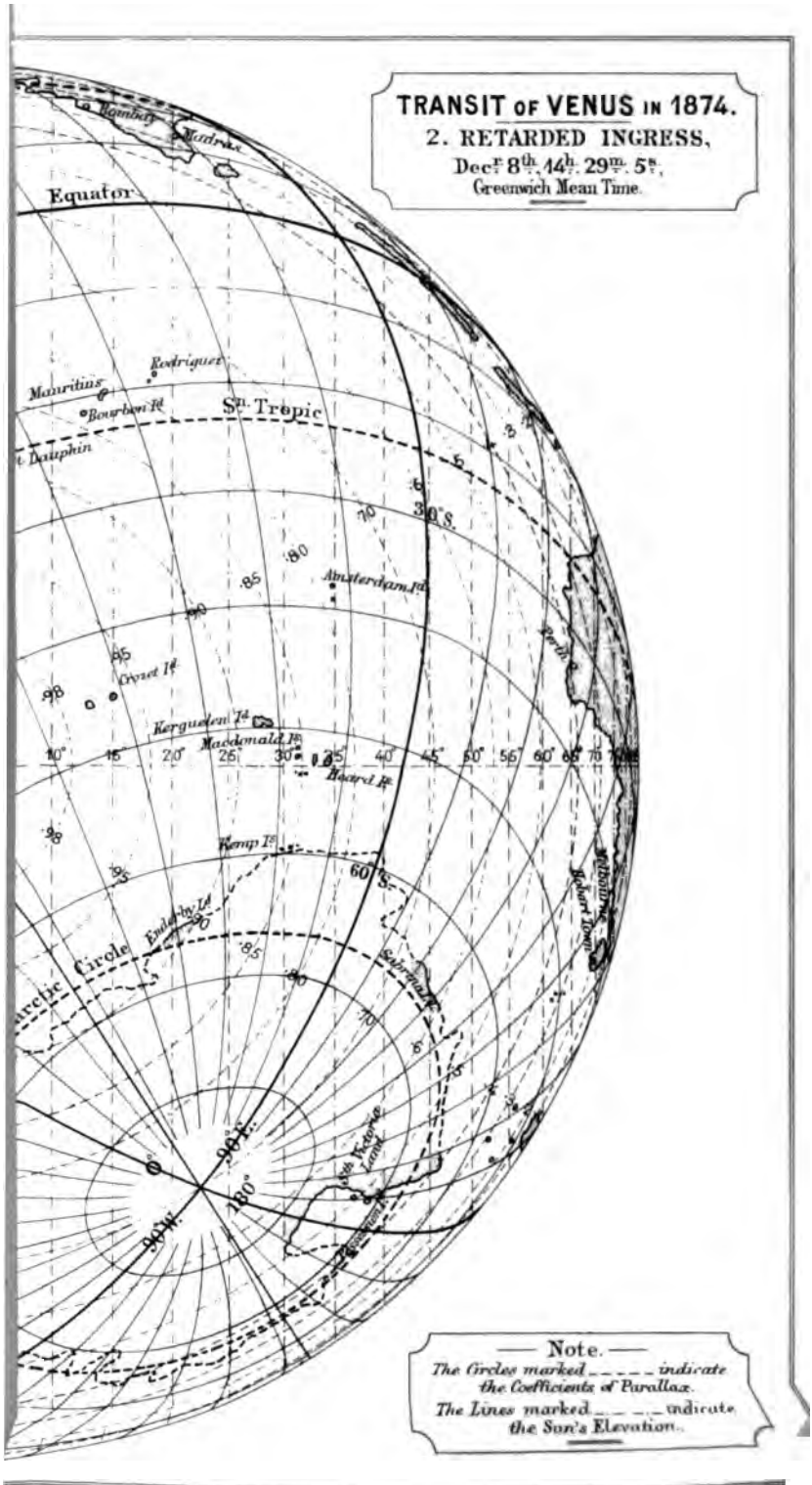
Table III. *Accelerated Egress.*

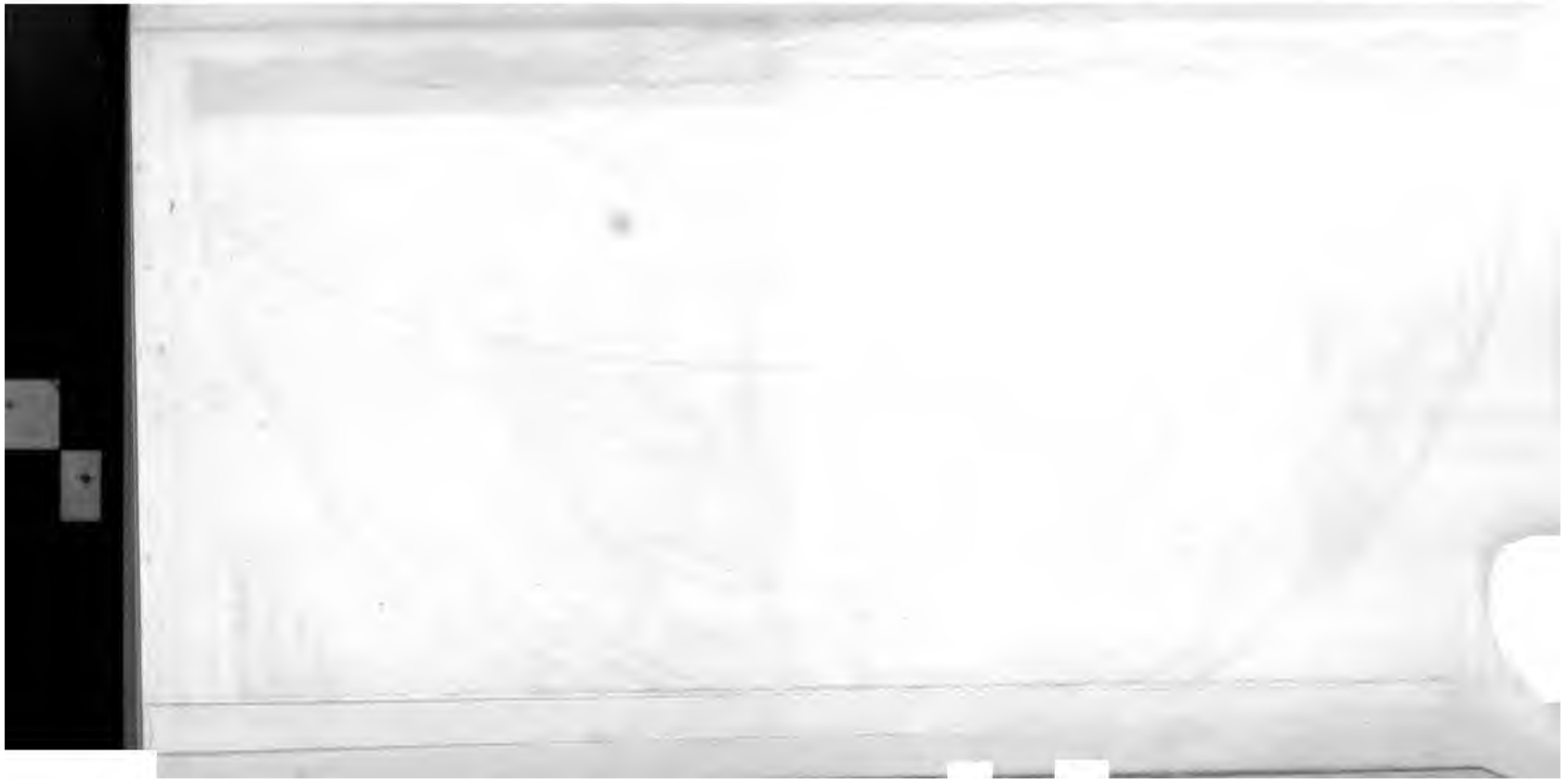
Station.	Sun's Elevation.			Coefficient of Parallax.		Acceleration in Minute	
	A	B	C	A	C	B	
	°	°	°			m	
South Victoria Ld. (Possession I.)	..	23·8	25·0	..	·89	10·3	1
Adelie Ld.	..	..	34·0	..	·83	..	1
Campbell I.	..	..	26·0	..	·79	..	1
Emerald I.	..	..	30·0	..	·78	..	1
Macquarie I.	24·0	..	32·0	·83	·75	..	
Chatham I.	11·5	..	16·0	·87	·75	..	
Canterbury (N.Z.)	19·0	..	22·5	·81	·71	..	
Wellington	17·0	..	20·0	·81	·70	..	
Sabrina Ld.	..	..	43·0	..	·70	..	
Enderby Ld.	..	41·0	39·0	..	·65	6·9	
Royal Co. I.	..	..	42·0	..	·64	..	
Auckland	15·0	14·4	19·2	·78	·64	8·6	
Kemp I.	..	..	51·0	..	·57	..	
Hobart Town	36·0	36·0	40·0	·66	·57	7·5	
Melbourne	38·0	..	43·0	·58	·48	..	
Sydney	33·0	..	37·2	·59	·48	..	
Adelaide	..	..	47·8	..	·40	..	
Kerguelen Ld.	..	60·5	57·1	..	·40	..	
Crozet I.	..	..	47·5	..	·34	..	
Perth (Aust.)	..	..	66·2	..	·25	..	

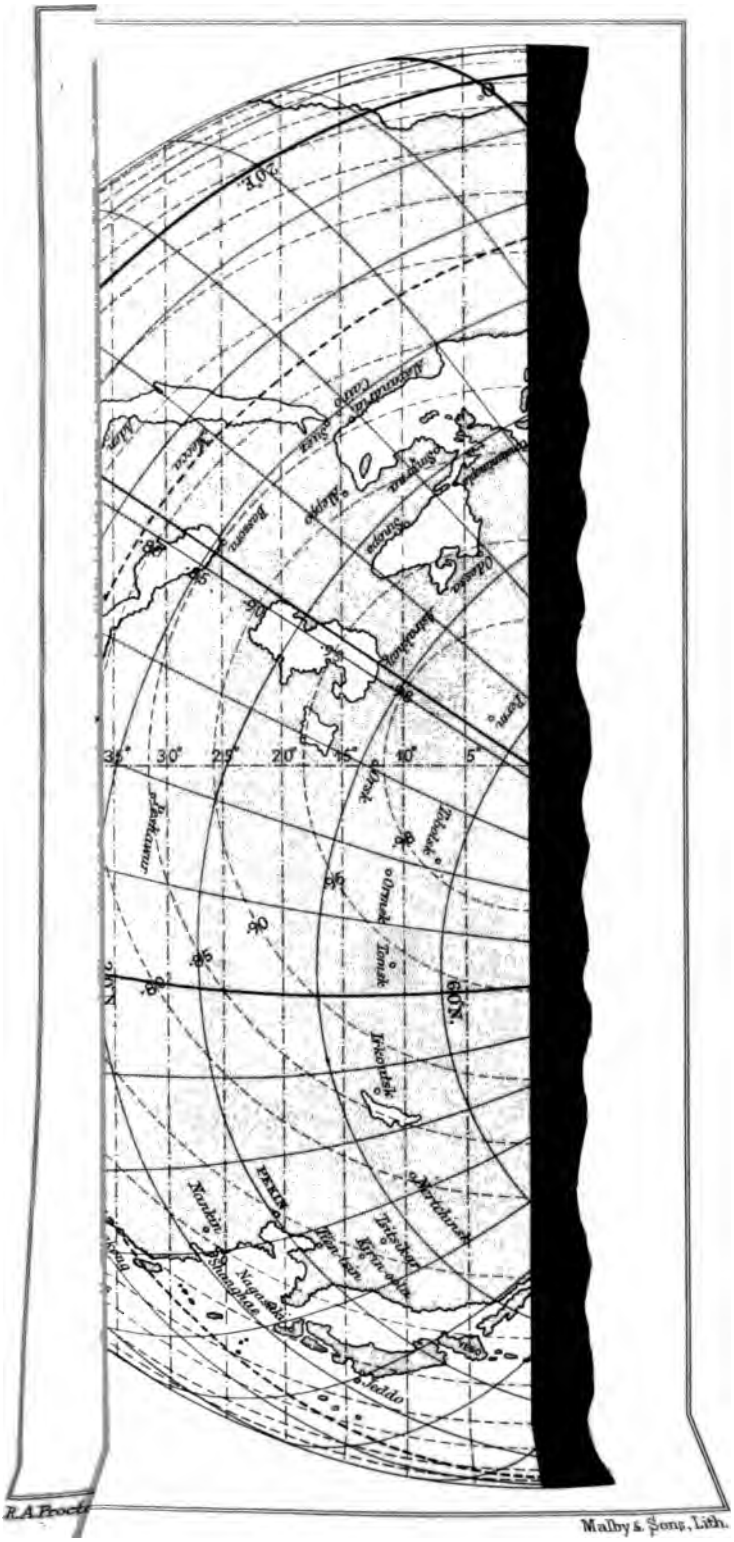
Table IV. *Retarded Egress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Retardat in Minu
	A	B	C	A	C	B
	°	°	°			m
Orsk	12·0	..	12·5	·98	·98	..
Omsk	12·5	..	11·5	·96	·97	..
Astracan	12·4	..	12·0	·97	·97	..
Aleppo	14·2	..	14·6	·91	·89	..
Peshawur	..	..	31·5	..	·85	..
Alexandria	13·0	..	14·0	·86	·84	..
Suez	16·0	17·6	16·1	·85	·83	9·4
Nertchinsk	..	..	10·1	..	·81	..
Delhi	..	..	38·0	..	·78	..
Tsitsikar	..	..	12·0	..	·72	..
Bombay	..	..	45·0	..	·70	..
Pekin	..	21·2	21·0	..	·70	7·7
Kirin-Oula	..	..	14·0	..	·69	..
Tien-tsin	..	..	17·1	..	·68	..
Calcutta	..	..	45·3	..	·68	..


TRANSIT OF VENUS IN 1874.
2. RETARDED INGRESS,
Dec.r 8th. 14h. 29m. 5s.
Greenwich Mean Time.
Equator
Bombay
Madras
Mauritius
Rodriguez
Bourbon Id.
Tropic
Dauphin
30° S.
Amsterdam Id.
Crozet Id.
Kerguelen Id.
Macdonald Id.
Heard Id.
Kemp Id.
60° S.
Enderby Id.
Circle
Melbourne
Hobart Town
Auckland
Note.
The Circles marked _ _ _ _ indicate the Coefficients of Parallax.
The Lines marked _ _ _ _ indicate the Sun's Elevation.

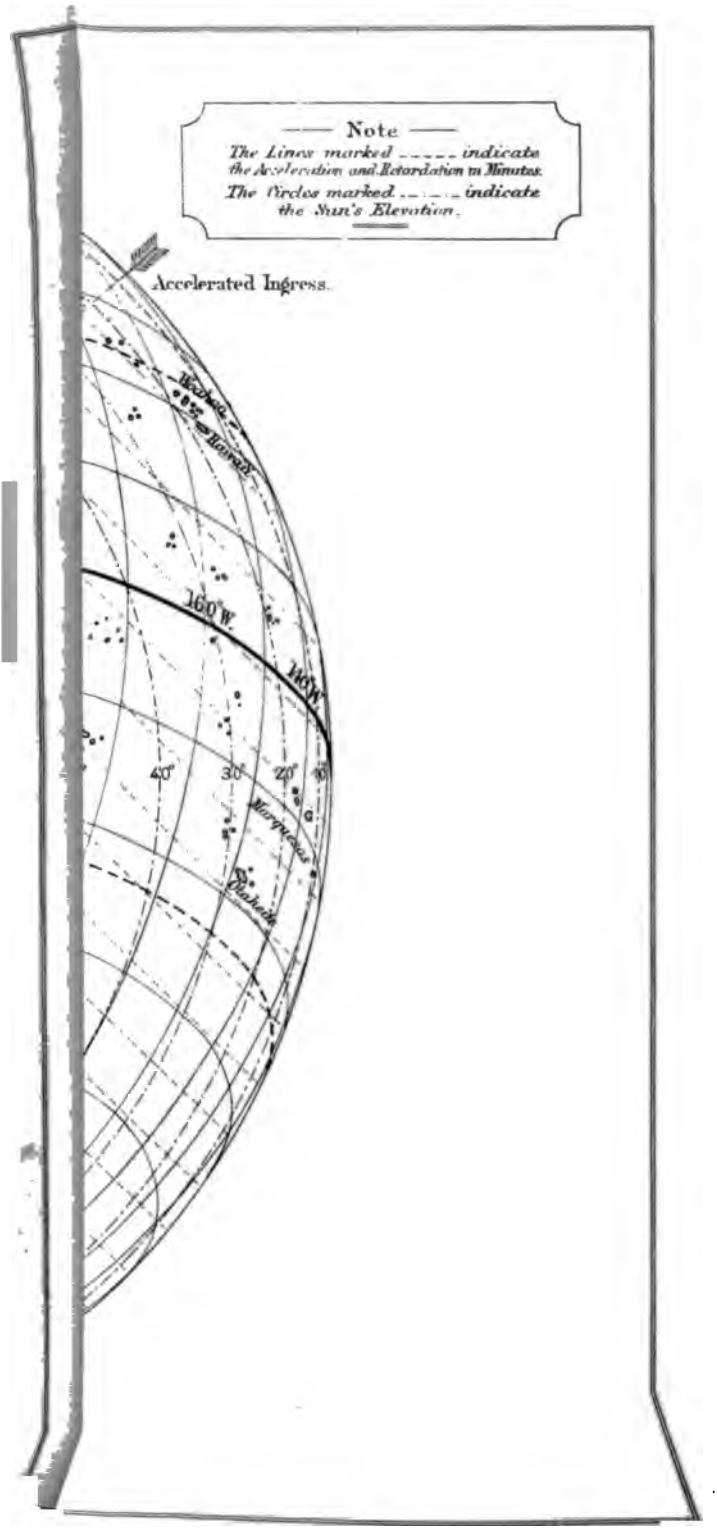

TRANSIT OF VENUS IN 1874.
2. RETARDED INGRESS,
Dec.r 8th. 14h. 29m. 5s.
Greenwich Mean Time.
Equator
Bombay
Madras
Rodriguez
Mauritius
Bourbon Id.
St. Tropic
Dauphin
30° S.
Amsterdam Id.
Crozet Id.
Kerguelen Id.
Maddemald Id.
Heard Id.
Kemp Is.
Arctic Circle
Emilie Id.
Saint Paul
Victoria Land
Melbourne
Robert Town
90° E.
180°
90° W.
Note.
The Circles marked _____ indicate the Coefficients of Parallax.
The Lines marked _ _ _ _ indicate the Sun's Elevation.





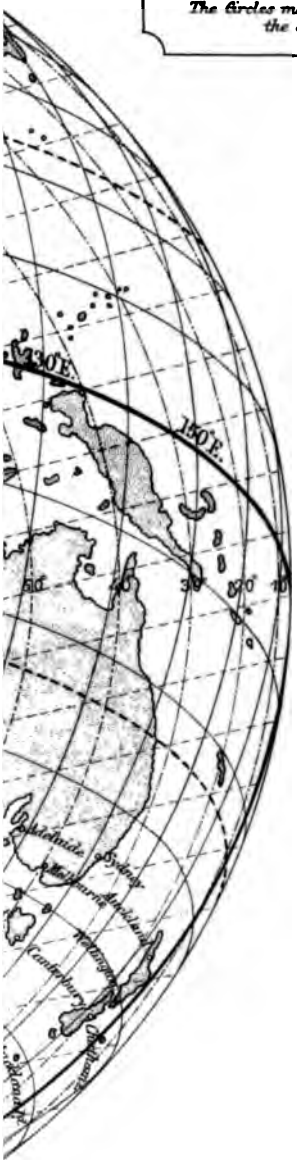
R.A.Procter

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Note
The Lines marked _____ indicate the Acceleration and Retardation in Minutes.
The Circles marked _ _ _ _ indicate the Sun's Elevation.
Accelerated Ingress.
Rochea
Hawaii
160°W.
140°W.
40°
30°
20°
10°
Marquesas
Otaheite

Note.

The Lines marked _____ indicate the Acceleration and Retardation in Minutes.
The Circles marked _ _ _ _ indicate the Sun's Elevation.



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Station.	Sun's Elevation.			Coefficient of Parallax.		Retardation in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
Aden	..	..	36·0	..	·66	..	7·8
Nankin	..	..	27·0	..	·62	..	7·6
Madras	..	..	52·0	..	·61	..	7·4
Shanghai	..	26·1	26·0	..	·57	6·2	7·2
Canton	..	..	37·0	..	·52	..	6·6
Hongkong	..	..	37·0	..	·50	..	6·5

It will be seen, on a comparison of tables A, B, and C, that the effects of the change of phase are in some cases important. The coefficients of parallax are affected in several instances by more than 0·1, and in two cases by 0·22. In the cases of Crozet Island (Table II.) and Chatham Island (Table III.) solar elevations are so improved, that these stations, which would have to be rejected if central passage were considered, are shown to be well-suited for the observation of internal contacts. The diminution of all the coefficients in Table III., through the change of phase, has an important influence on the value of Delisle's method, so far as egress observations are concerned. It is important to notice, also, that under heads C in Tables III. and IV. many stations not hitherto recognised as available are included among the best places for observing egress. The Indian stations in Table IV. seem too valuable to be neglected. Peshawur is better even than Alexandria; Delhi is not inferior to the latter station (when solar elevation is considered as well as coefficient of parallax). Bombay, Calcutta, and Madras are also excellent. It may be noticed also that Bombay and Madras, which, when considered with reference to central passage, had seemed suitable places for the observation of retarded ingress, are found to have so poor a coefficient of parallax when reference is made to internal contacts, that it would be useless to observe ingress there (so far at least as the application of Delisle's method is concerned).

Of course, it will be impracticable for this country to send observers to more than a certain number of stations. But it is not unlikely that besides Russia, France, and England (the only countries specially concerned in the transit of 1874), other nations may care to take part in the solution of the noble problem of determining the Sun's distance; and thus it seems advisable that all the stations where there will be any chance of obtaining useful observations, should be tabulated as nearly as possible according to their relative values.*

* It may not be amiss to point out that the numbers tabulated under head C in the above tables may be readily tested in a few minutes for any assigned station. Indeed, the whole work of determining the circumstances of the transit for internal contacts, as seen from any station, may be gone through in about an hour, and the main part of the work thus gone through would not require to be repeated for other stations. Although I have no reason to doubt the accuracy of the results tabulated above, it may be well that they should be checked.

As regards the comparison between Delisle's and Halley's method for the transit of 1874, I may remark that M. Puiseux's results seem to have been somewhat misinterpreted. He does not anywhere speak of Halley's method as the best, but simply states that he can see no reason why it should not be applied: nor do his figures establish the superiority of Halley's method.

I believe I shall be able to show, however, that there is at least a possibility that Halley's method may be so applied to the transit of 1874, as to give absolutely the best means of deter-

The justice of the following processes will be obvious to every one acquainted with the principles on which the transits of an inferior planet depend:—

Take out from the *Nautical Almanac* and tables of the planetary elements,—

App. Semi-diameter of Sun on December 8 ..	= D
,, ,, Venus in inf. conj. ..	= V
Horizontal Parallax of Venus (in inf. conj.) refd. to Sun	= P
Sun's Southern Declination on December 8 ..	= δ
Equation of Time	= ϵ

On any convenient scale describe concentric circles with radii

(i.) D (ii.) D + V (iii.) D − V
(iv.) D − V + P (v.) D − V − P

On circle (ii.) take (according to Mr. Hind's elements of the transit) position-angles 130°·7 from South towards East, and 160° from South towards West, and join the points thus determined. Determine with a protractor the position-angles Q_1, Q_2, Q_3, and Q_4, for the intersections of this chord with the circles (iv.) and (v). Again, making the extremities of the chord correspond to hours $13^h\,47^m$ and $18^h\,26^m$ (from Mr. Hind's elements of the transit) divide the chord into hour-divisions. Thence determine epochs H_1, H_2, H_3, H_4, corresponding to the intersections of the chord with the circles (iv.) and (v.).

Now adjust a globe so that, looked at from above (vertically), its appearance shall be that presented by the earth, supposed to be seen from the Sun, at apparent solar time $H_1 + \epsilon$, and with southerly solar declination δ. From the southern intersection of the brazen meridian with the horizon-circle, take an arc of Q_2 towards the East along the latter circle. This will give the place where ingress is most accelerated. To determine the value of any station near this point, measure the arc-distance of the station from the point (α), and from the horizon-circle (β). Then cos α = coefficient of parallax, and β = Sun's elevation. And thus the coefficients and solar parallax may be determined for any number of stations, with corresponding methods for retarded ingress and for egress.

It may be necessary to notice that, to convert coefficients of parallax into minutes of acceleration or retardation, reference must be made to the intervals of maximum acceleration and retardation. These are, for accelerated ingress and retarded egress (Tables I. and IV. respectively) $11^m\,58^s$; for retarded ingress and accelerated egress (Tables II. and III. respectively) $13^m\,8^s$. A coefficient of parallax p corresponds for the former pair of cases to a time-interval

$$[11^m\,58^s - 35^s\,(1-p)]\,p,$$

for the latter to a time-interval

$$[13^m\,8^s + 35^s\,(1-p)]\,p.$$

mining the Sun's distance available before the transit of 2004 (about the circumstances of which I know nothing). It only requires that the same energy should be devoted (either by England or some other nation) to the coming transit, which has been called for in relation to the transit of 1882. There will also be the same chance of failure in one case as in the other.

To prove the justice of these views I point to Nertchinsk (or its neighbourhood) as a suitable station for observing the lengthened transit, and the neighbourhood of Enderby Land as a proper station for observing the shortened transit. The above Tables give for the former station a lengthening of $(5^m{\cdot}7 + 9^m{\cdot}8)$ or $15^m{\cdot}5$; and for the latter a shortening of $(11^m{\cdot}8 + 8^m{\cdot}5)$ or $20^m{\cdot}3$. The total difference of duration is thus shown to be $35^m{\cdot}8$. Setting against this the best case for Delisle's method (Woahoo from Table I. and Crozet Island from Table II.), we get the total difference of absolute time, is $(11^m{\cdot}2 + 12^m{\cdot}6)$ or $23^m{\cdot}8$. And the relative values of the methods (in these cases the most favourable for each) are given by the formula

$$\frac{\text{Halley's}}{\text{Delisle's}} = \frac{(35{\cdot}8)^2}{(23{\cdot}8)^2}\,\frac{\{(4{\cdot}28)^2+(1)^2\}}{(4{\cdot}28)^2}\ast$$

$$= \frac{2477}{2075}\dagger \text{ approximately;}$$

and in this ratio, independently of all the other advantages which it presents, does Halley's method surpass Delisle's. By taking Kemp Island, Adelie Land, Victoria Land, Crozet Island, or Kerguelen's Land for the southern, and Tsitsikar, Kirin-Oula, or Tien-tsin, for the northern stations, we get differences of duration ranging from 33^m to $30^m{\cdot}6$, corresponding (according to Mr. Stone's formula) to a range from 24^m to $22^m{\cdot}2$, in the case of Delisle's method. There is little chance of the latter method being applicable with a difference greater than the lowest of these methods; and the highest is a difference which the best stations will not give by Delisle's method.

When it is remembered that Halley's method is so much the simpler, and that stations selected with reference to it give a double chance of at least a useful observation, the above considerations seem to decide the question of the relative values of the two methods in 1874.

* Here I have used the values given in Mr. Stone's paper (*Monthly Notices*, April 8, pp. 251, 252). He would have obtained a larger value for e had he calculated for internal contacts. This would, of course, have been a change unfavourable to my case. But against this I set the fact that his careful examination of the circumstances of internal contacts will tend to render the probable errors much smaller (relatively) in 1874 than they were in 1769, when the observers had very vague notions of the phenomena they were to expect and to watch.

† It would not be correct to reduce the number of minutes 35·8 in a certain proportion, and take the excess over $23^m{\cdot}8$, as measuring the superiority of Halley's mode. The proper way of treating the question is to indicate the relation between the two modes by a ratio, as above.

Lastly, as to the relative values of the transits of 1874 and 1882, considered with reference to Halley's method.

Mr. Stone's remark, that the choice of stations is limited to those at which the Sun will have an elevation of at least 10°, reduces the maximum available difference of duration in 1874 from $36\frac{3}{4}^{m}$ (the value I had before mentioned) to $35\frac{3}{4}^{m}$. But the transit of 1882 is much more seriously affected. The suggested station near Sabrina Land must be rejected at once. And, although the suggested station near South Victoria Land corresponds to an elevation of 10° at ingress (see map 5, *Monthly Notices* for December last), there is no accessible spot in that neighbourhood which will give any such elevation. At Possession Id. the Sun's elevation will not be much more than 5° at ingress; and at Coulman Island, the most southerly station which Antarctic seamen hope to reach, the Sun's elevation will be but 7°; and even if these islands were suitable, they give a difference of duration perceptibly less than that which I had dealt with in my former paper.

I must add that I had fully taken into account the difference in the clinging of the disk of *Venus* to the Sun's limb in 1874 and 1882. Indeed, I had adopted a considerably greater proportion than that indicated by Mr. Stone. But, as the principle he lays down requires that both Sabrina Ld. and Victoria Ld. should be dismissed from our consideration in 1882,—and as there is absolutely no other southern station at all comparable with these two, as far as the lengthening of the transit's duration is concerned, we seem forced to the conclusion that if Halley's method fails totally for either of the coming transits, it is for that of 1882 and not for that of 1874.

On the general question of the value of the transit of 1874, I find myself compelled to adopt a different opinion from that which Mr. Stone has expressed. Although the comparative slowness of ingress and egress increases the probable error, it increases the available time-differences in at least the same proportion,—I say, *at least*, because I cannot believe that the errors of an observation of ingress or egress increase fully in the proportion of the slowness with which the phenomena succeed each other. But even assuming this unfavourable view, it may yet be shown that, *cæteris paribus*, slow transits (*i. e.* transits of short chord) are as valuable as more rapid ones when Halley's method is considered, and more valuable when Delisle's method is considered. For, in the first case, we have

(1), Value of a transit varies as maximum observable difference;

(2), Maximum observable difference varies as slowness of rate of ingress or egress* (*cæteris paribus*);

(3), Value of a transit varies as $\frac{1}{\text{Slowness of rate of ingress}}$;

* Estimated, of course, perpendicularly to the limb.

therefore, for transits of different rates, but having circumstances otherwise equal, the value is appreciably constant, so long as the chord of transit is not too near a diameter of the Sun.

For the second case we have (1) and (2) unaltered, but in place of (3) we get the

$$\text{Relative value of transit varies as } \frac{1}{\left(\frac{k^2}{v^2}\right) + l^2};$$

where k is some constant, v represents the rate of ingress or egress, and l is the error in determining longitude of a station. Thus, the value of a transit varies as $\frac{1}{v^2} \div \left(\frac{k^2}{v^2} + l^2\right)$ or inversely as $(k^2 + v^2 l^2)$, and the slower transits are therefore the most favourable.

It is worthy of notice, however, that, *as a rule*, when there are two transits separated by eight years, the first is the less favourable for the application of Halley's method. The present case shows that to this rule there are exceptions.

Field Notes on the Zenith Telescope. By G. Davidson, Esq.

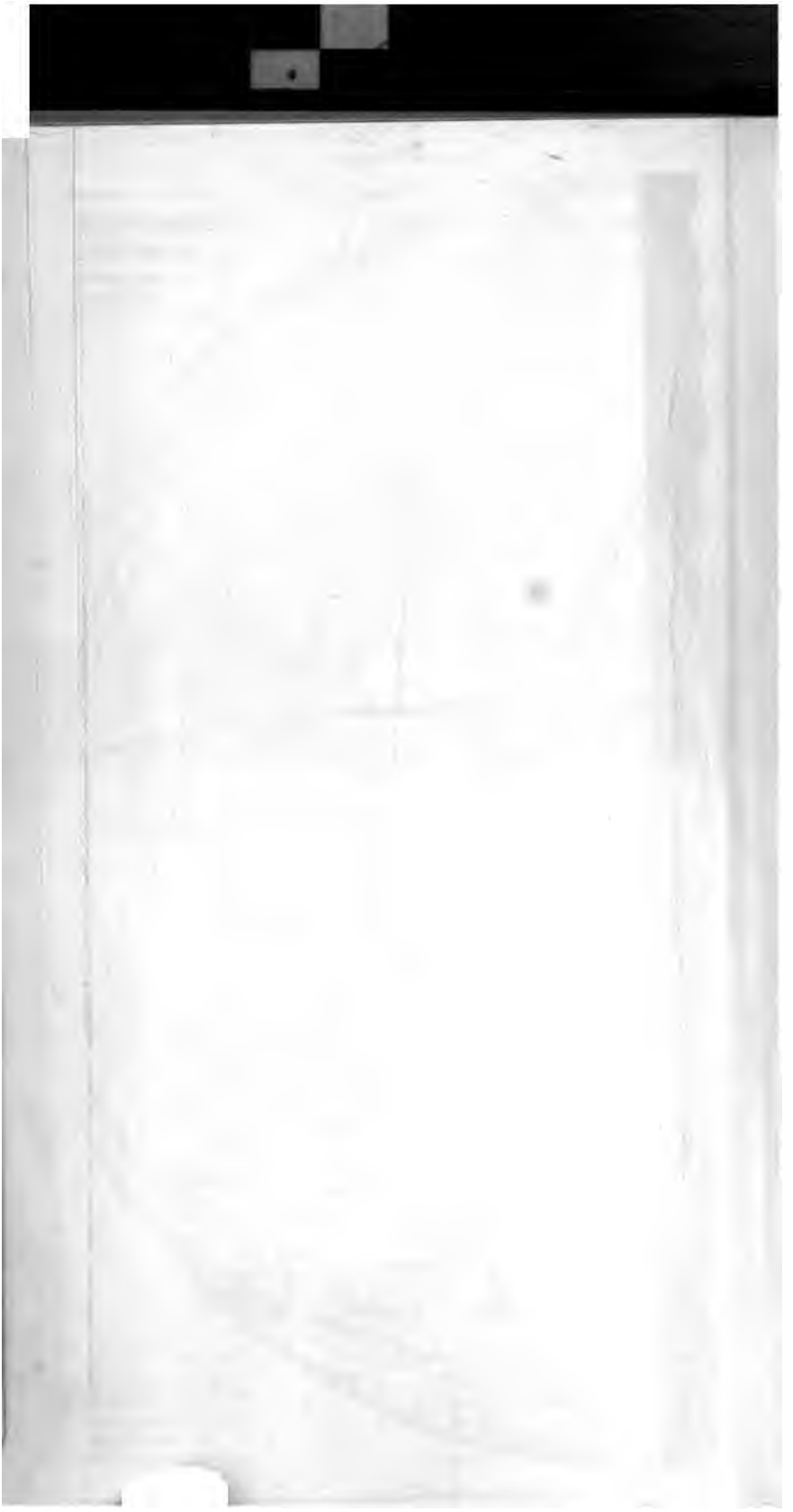
(*Communicated by Prof. Piazzi Smyth.* Abstract.)

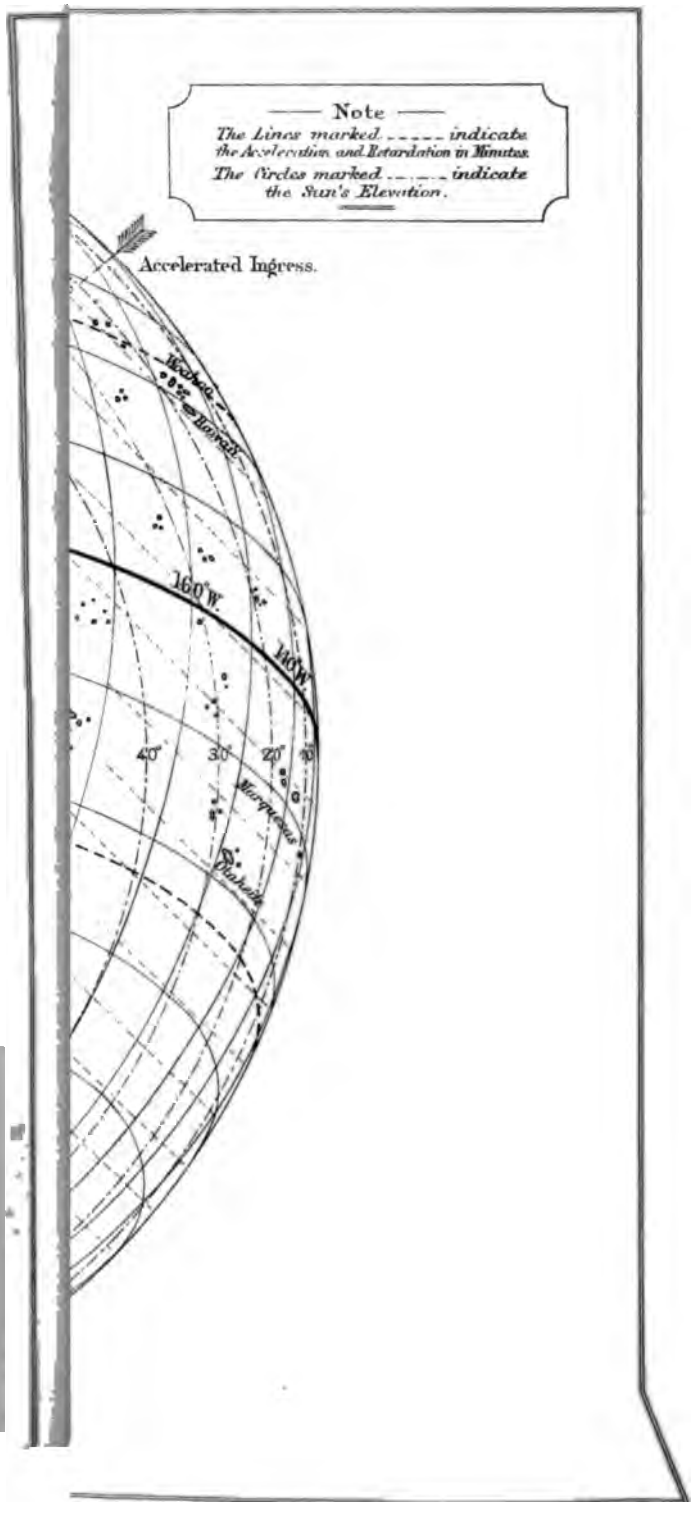
The principle of the method of determining the latitude with the Zenith Telescope is based upon the proposition that when the meridional zenith distances of the two stars at their upper culmination (one being north and one south of the zenith of a station) are equal, the colatitude is the mean of their north polar distances.

When the zenith distances of the pair of stars differ by an arc less than that subtended in the field of view of the telescope, that arc is measured by means of the micrometer attached to the telescope; and, at the times of observation, the error in direction of the vertical axis of the instrument in the plane of the meridian is measured by a delicate level.

The micrometer and level are therefore essential means by which the principle of the method is reduced to a practical field application: whilst the generally unsatisfactory correction for refraction in observations depending on the measure of simple zenith distances is avoided by applying the differences of refraction for the small difference of zenith distance of the two stars.

The paper contains a full discussion (illustrated by examples) of the methods of ascertaining the instrumental errors and other corrections as used in the U. S. Coast Survey; and of two different methods used for reducing the observations; and it is shown how the instrument may be used for extra-meridian observations for latitude, &c., which observations, by means of known stars too far apart for the application of the preceding accurate methods, may (it is remarked) be often used with advantage on a geographical reconnaissance or for some special object.




Note
The Lines marked _____ indicate the Acceleration and Retardation in Minutes.
The Circles marked _ _ _ _ indicate the Sun's Elevation.
Accelerated Ingress.
Woolwich
Barnet
160° W.
140° W.
40°
30°
20°
Margate
Dublin

matter appear to move across a spot without producing any permanent alteration."*

These observations are in entire harmony with my opinion that these bridges are portions of faculæ, and with the confirmatory observations of Mr. Huggins and Mr. Browning, of the great brightness of some bridges.

It is requisite that I should here remark that I confine this interpretation to the phenomena for which I adopt the original designation by Sir J. F. W. Herschel, of "*bridges of light;*"† not extending it to the other "bridges" seen to connect the opposite sides of a spot, the constitution of which is manifestly the same with that of the penumbræ, and which have every appearance of being at the same level with them. This discrimination is in agreement with the inferences of the Kew observers already stated.

From the structure of the two bridges on the spot observed by Mr. Browning on March 14, which, as described by him, was evidently the characteristic structure of penumbræ, while yet the bridges, it is said, were the brightest parts of the object represented, it is probable that in his observation the two kinds of bridges were optically confounded together, on account of the partial superposition, in this instance, of bridges of light over penumbral bridges.‡

* *Solar Physics*, First Series, p. 31, sect. 27. In a note on the fact last described I have suggested that these detached portions of luminous matter are, perhaps, some of the hypothetical bodies which I have termed *meteoritic masses*. After considering, however, the observations, both telescopical and spectroscopical, since made of the solar disk, I no longer retain this idea.

The term "meteoritic masses" is nearly equivalent to that of *meteoroids*, used by Professor H. A. Newton to designate the bodies which become sensible to us as meteors, but which, until recently, were otherwise utterly unknown, though their existence was certain, and had been the subject of several reasonable hypotheses. Now, however, we have grounds for believing, according to the judgment of astronomers in general, that they are related to comets, and, in my own opinion, that they have also a genetic relation to the Sun. See *Companion* for 1866, pp. 91–95, and for 1868, pp. 125–126; also, *Proceedings of the Royal Society* (vol. xiv. pp. 120–129) for March, 1865, and *Report of the British Association for the Advancement of Science* for that year, Appendix to Report of Committee on Luminous Meteors, pp. 130–131, and 140–142.

† See *Results of Astronomical Observations made at the Cape of Good Hope;* "Observations of the Solar Spots," p. 435 (par. 428). The accompanying plate (xvii.) presents, on a small scale, in figs. 4, 6, 7, 13, characteristic curved, if not spiral examples, of these objects.

‡ Graphical representations of celestial objects should, of course, describe with all possible fidelity what the observer believes that he sees, leaving the due interpretation of the several appearances which make up the visible projection for future scrutiny. But it would appear to be desirable that telescopic observers of the Sun should now endeavour to discriminate between the two kinds of "bridges," and to ascertain the respective limits of each, where they are not superposed. In many figures of solar configurations both appear to be represented, but as if they were continuous portions of the same physical objects, differing only in brightness.

In observing the Sun in the profound manner, so to speak, which, in the actual condition of heliography and solar physics, astronomers are called upon to adopt, "the combination of the photographic record" of the telescopic pro-

The varying forms of the bridges of light, observed—always within the penumbral area, and commonly at its inner edge bordering the umbra, but sometimes crossing the latter,—on the remarkable spot seen in October, 1865, as described and figured by the Rev. F. Howlett,* are also strictly conformable with the view of their nature now taken. They are such as would be presented by the extended vertices of a spiral or curved sheet or volume of flame or incandescent gaseous matter, rising from the area of the umbra. We see in them, in all probability, the upper termination (as a physical object, neglecting at present the consideration of its chemical nature) of the vorticose or cyclonic torrent, of which, as viewed in horizontal section, the umbra of the spot is merely the visual projection. In all probability, again, such torrents are liquid below and aeriform above, a constitution which would account for some apparent inconsistencies in the recorded spectroscopical observations of the several physical elements of spots. The spectra of bright lines seen over umbræ, both by Mr. Huggins and Mr. Lockyer, would naturally be caused by their upper gaseous portions; a subject to which I have alluded in the *Monthly Notices* for January.

I intended now to discuss, as then intimated, the mutual relations of Sun-spots, faculæ, and prominences, considered as the results of one continued action, beginning in the nucleus of the Sun, including the compatibility of that induction with the local distribution of the spots; and also the character of the markings by which the faculæ-prominences may be expected to be recognised in photograms of the solar surface viewed in the stereoscope. But I must reserve both subjects for the next session of the Society.

I may remark, however, that the recent spectroscopical observations of Mr. Lockyer† and Padre Secchi,‡ assuming their accuracy, are conclusive as to the identity of faculæ and prominences as portions of the same physical objects, while they are in perfect accordance, also, with the theory of the constitution of

jection with "the stereoscopic examination of it" will be invaluable. I formerly remarked that, "Admitting the powers of" this method of research "to be correctly estimated by Mr. De La Rue, and assuming that there is no source of fallacy in its use, it may be doubted whether any other means at present known to science will so adequately avail for the discovery of the order of superposition of the several parts or objects of which a given area of the visible photosphere consists, or their arrangement in altitude vertical to the Sun's surface."—*Companion*, 1864, p. 40.

Mr. De La Rue's observations of the floating faculæ, which I have discussed in the *Monthly Notices* for January, and those of the nearly related phenomenon of the comparative elevation of bridges of light, cited from him and the other Kew observers in the present paper, are accordant examples of the value of this combined method in astronomical research, a crucial addition to which is now afforded by the spectroscope.

* *Monthly Notices*, November, 1865, pp. 12–20, figs. 5 to 10.

† *Proceedings of the Royal Society* (vol. xvii. pp. 415–418) for April 29, 1869.

‡ *Comptes Rendus* (vol. lxviii.), May 10, 1869, p. 1082. The entire contents, however, of P. Secchi's two papers here published bear immediately on the objects of this reference.

the spots which I announced in a paper, entitled "Inferences and Suggestions in Cosmical and Geological Philosophy," inserted in the *Proceedings of the Royal Society* for March, 1865. This would require that the spectrum of the umbra of a spot should not exhibit any line of absorption which is not present in that of the Sun's normal surface, and also that no ordinary solar line should be wanting in it; facts actually observed by Mr. Huggins;* with whose results, in this respect, those obtained by his contemporaries are in agreement.

London Institution, June 10, 1869.

On Solar Currents. By C. H. Weston, Esq.

(*Extracts from Note-Book.*)

1869, June 4th. Large spot changed since the 2nd. Penumbra with increased margin, especially on S. shore of umbra.—5th June. A large luminiferous promontory running out from E. side of S. shore with extremity expanded. Shut off Sun's disk by diaphragms. Now the expanded or club end was seen bifurcated, and of the two branches the W. one longest. In afternoon W. branch more elongated, and also with branchlet at right angles to the branch. Towards end of branchlet two little buddings like incipient transverse arms of a Roman cross.

One end of this luminous promontory was attached to the penumbra, and not to the overlying photosphere. The margin of the penumbra was so broad on the S.E. side that not only was the connexion apparent, but the penumbra itself visibly intervening between the penumbral promontory and photosphere. Thus, while the S.E. extremity was undoubtedly penumbral (physically and chromatically) the other was as brilliant as the photosphere itself and the terminal point especially so; preponderating evidence that the N.W. end was in the act of being lifted up into the upper regions of the photosphere.

6th June. Same branch and branchlet increased in breadth and now pretty uniformly broad, and reaching to W. margin. Perceived well by strong contrast of subjacent umbra, that the promontory was unquestionably *arched* on the S.E. side, while the N.W. end appeared to touch the photosphere and partook of the same brilliancy; further confirming the idea that the part of S.E. penumbra was arched up (from rising currents) with its N.W. end on the *higher* photospheric elevation.

7th June. Examined another spot. No penumbra apparent on the S.E. side. Faculous matter (clearly part of the photosphere)

* See *Phil. Trans.* 1868, p. 553.

extended across the umbra. On N.W. side the penumbra was visible on each side of the jutting cloud. Towards N.W. the extremity was dichotomized by a black line (recalling *Saturn's* ring) evidently showing umbra beneath. Three hours later the cloud lengthened, but throughout the entire length same lustre at photosphere, showing an approximate horizontal movement from S.E. to N.W.

Another spot examined, showing a bridge truly penumbral, extending from E. to W. margin of umbra. In the afternoon the *bridge more brilliant*, and so also the *inner margin of the penumbra* (in contact with the umbra), while the external margin was not so illuminated,—evidence of upward movement both in the bridge and peripheral parts of penumbra bordering on the umbra.

These three cases seem to show locally-upward and horizontal currents.*

I have given these descriptions as concisely as possible, and will not, therefore, add any remarks on luminosity. But I must confess that I concur with M. Faye in thinking there is no necessary connexion between the degree of photospheric luminosity and solar caloric.

Ensleigh Observatory,
Lansdowne Bath, June 1869.

Probable Errors of Greenwich Observations in Zenith Distance, estimated by mere discordances from the separate means. By E. J. Stone, Esq.

No.	Z.D.	P.E.	No.	Z.D.	P.E.
	°	″		°	″
1	0	0·47	14	44	0·59
2	26	0·48	15	45	0·60
3	28	0·49	16	46	0·61
4	30	0·50	17	47	0·62
5	32	0·51	18	48	0·63
6	34	0·52	19	49	0·64
7	36	0·53	20	50	0·65
8	38	0·54	21	51	0·66
9	39	0·54	22	52	0·67
10	40	0·55	23	53	0·68
11	41	0·56	24	54	0·69
12	42	0·57	25	55	0·70
13	43	0·58	26	56	0·71

* Used (full apertures) two Newtonian Equatoreals, 9·0 and 14·15 inches, and 8·75 and 16·5 feet respectively.

No.	Z. D.	P. E.	No.	Z. D.	P. E.
	°	′		°	′
27	57	0·72	42	72	0·87
28	58	0·73	43	73	0·89
29	59	0·74	44	74	0·92
30	60	0·75	45	75	0·95
31	61	0·76	46	76	0·98
32	62	0·77	47	77	1·02
33	63	0·78	48	78	1·08
34	64	0·79	49	79	1·14
35	65	0·80	50	80	1·20
36	66	0·81	51	81	1·28
37	67	0·82	52	82	1·38
38	68	0·83	53	83	1·52
39	69	0·84	54	84	1·68
40	70	0·85	55	85	1·92
41	71	0·86	56	86	2·29
			57	87	3·32

The number of observations employed in obtaining these probable errors is more than 2000.

The probable errors were first determined for ten principal points, and a curve was traced amongst these points: the results given are those read off from the curve. Observations of *Sirius* and *Rigel*, which are incessantly observed at Greenwich, and observations of which are often made under unfavourable circumstances and in the daytime, have rather larger probable errors than the general average, and have not been included in the results.

Occultations of Stars by the Moon. By John Joynson, Esq.

I have not had an earlier opportunity of seeing Mr. Plummer's paper remarking on one of mine on "Occultations of Stars by the Moon," in vol. xxix. No. 5.

I am afraid that I have given Mr. Plummer a considerable amount of trouble; but my apology, if he considers one needful, must be that I found that sometimes, when the disappearances and reappearances occurred at or about the same angle, there was a greater difference than seemed to be accounted for by the parallax.

I am glad, however, to find that I was mistaken, and that the calculated times agreed so well with the observations I sent to the Society, with the exception of that of the reappearance of 120 *Tauri*.

Occultations of Stars by the Moon.

1869.			G.M.T. h m s	
19 Feb.	48 Tauri	Disappearance	9 4 40·3	cloudy at reappearance.
23	ζ' Cancri	,,	10 11 40·2	,,
24	π^3 Cancri	,,	10 45 16·9	
		Reappearance	11 43 3·5	
22 March	*g* Geminorum	Disappearance	9 17 16·7	
		Reappearance	10 26 21·8	
16 April	119 Tauri	Disappearance	7 29 44·2	
		Reappearance	8 23 19·8	
	120 Tauri	Disappearance	8 18 25·6	
		Reappearance	8 52 48·5	
18 May	Regulus	Disappearance	9 54 27·9	
		Reappearance	10 7 25·2	

Latitude 53° 28′ 24″ N.
Longitude $0^h\ 12^m\ 7^s$ W.

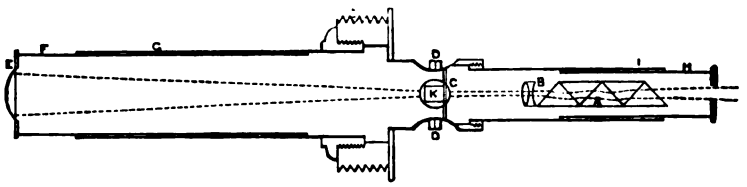
Waterloo, near Liverpool,
3 *June*, 1869.

On a Simple Form of Star Spectroscope.

By John Browning, F.R.A.S.

A simple, efficient, and economical spectroscope for Astronomical work has long seemed to be a desideratum.

The instrument which I have the honour to exhibit and describe to the Society will, I think, be found better adapted for use with telescopes of moderate aperture than any other contrivance hitherto proposed.



In the diagram, A is a compound direct-vision prism consisting of five prisms, B is an achromatic lens which focusses on the slit C, by means of a sliding-tube H, both the prisms and lens are fastened in this tube; K is a small right-angled prism covering half the slit, by the aid of which light may be seen reflected through the circular aperture in front of it.

In this manner a comparison may be made with the spectra of metals or gases. The reflecting prism, with the ring to which it is attached, can be instantly removed and the whole length of the slit used if desired. D D is a ring milled on the edge, on turning this round both edges of the slit are made to recede from each other equally, being acted on by two hollow excentrics. The lines can thus be increased in breadth without their centres being changed, a point of importance. E is a cylindrical lens attached to the tube F, which slides in another tube G.

To use the spectroscope the adapter L, which is the same size as the Society's thread, has only to be screwed into the eye-drawer of the telescope in the place of the ordinary Huyghenian eye-piece.

The drawer must then be adjusted so that the slit G comes exactly to the focus of the object-glass. This should be tried beforehand, by getting an image of the Sun and then making a mark on the drawer-tube of the telescope. When this has been done the tube can be set by this mark, and the spectroscope screwed into it at any time without any trouble in adjustment.

If the cylindrical lens be removed the spectrum of a star will be a mere line of light.

The cylindrical lens is for the purpose of widening this line to such an extent that the lines in the spectrum may readily be discerned. For this purpose the lens must be placed *with its axis at right angles with the slit*, and the best distance from the slit will be between three and six inches. The nearer it is brought to the slit the broader will be the spectrum, but it should not be used too close, on account of the diminution of the light.

When it is desired to obtain the spectra of planets, comets, or nebulæ, or indeed any heavenly bodies possessing considerable diameter in the telescope, the cylindrical lens may be dispensed with advantageously.

The weight of the instrument, as I have figured and described it, being only about seven ounces, much less than an ordinary micrometer, it will not seriously affect the balance of a telescope.

Various portions of this apparatus have been for some time used by different observers. The arrangement of the whole in combination with the new slit is the only novelty in the contrivance.

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MONTHLY NOTICES

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VOL. XXIX. *Supplementary Notice.* No. 9.

On the Cluster in Perseus. By Dr. O. Pihl.

In the Supplementary Number of the *Monthly Notices* for 1868 was inserted a paper of mine, on the examination of one of the clusters in *Perseus*.

In preparing for publication a detailed account of the subject, I have detected an error in one of the coefficients employed for the reduction of the declinations of the stars, which has caused a systematic error in that co-ordinate, making it too great in some cases by about 1″·5.

Since the paper in question was published, the number of observations has been increased for the declinations of a few stars, which offer peculiar difficulties of observation, and some slight modification in the mean of their declination has, therefore, been obtained.

The following list gives the corrected *relative* values in declination for 85 stars measured, as referred to the star No. 25 in the group:—

No.		No.		No.	
1	+ 759·77	8	+ 816·73	15	+ 1394·77
2	+ 830·24	9	+ 1119·64	16	+ 472·04
3	+ 948·71	10	+ 1380·16	17	+ 1458·96
4	+ 432·36	11	+ 258·30	18	+ 1021·76
5	+ 1250·69	12	+ 300·01	19	+ 806·23
6	+ 354·56	13	+ 1442·02	20	+ 1378·18
7	+ 995·04	14	+ 1387·26	21	− 177·88

No.		No.		No.	
22	= + 805″·59	44	= + 577″·88	65	= + 390″·80
23	+ 895·22	45	+ 700·07	66	+ 319·27
24	− 120·94	46	+ 555·66	67	− 34·93
25	·00	47	+ 384·94	68	+ 480·22
26	+ 1483·48	48	+ 668·70	69	+ 77·33
27	+ 1028·90	49	+ 104·51	70	− 95·94
28	+ 403·40	50	+ 987·12	71	+ 820·11
29	+ 642·24	51	+ 1039·42	72	+ 1096·72
30	+ 440·39	52	+ 1035·67	73	+ 1515·15
31	+ 639·40	53	+ 34·78	74	− 53·93
32	+ 962·33	54	+ 605·16	75	+ 54·65
33	+ 1490·75	55	+ 29·96	76	+ 1304·00
34	+ 287·51	56	+ 598·93	77	+ 930·17
35	+ 708·87	57	+ 34·30	78	+ 71[illegible]·72
36	+ 716·84	58	+ 1370·13	79	+ 485·43
37	+ 223·61	59	+ 5[illegible]·17	80	+ 659·27
38	+ 560·53	60	− 1·03	81	+ 742·46
39	− 277·01	61	+ 1158·77	82	+ 114·32
40	− 165·15	62	+ 859·01	83	+ 642·28
41	+ 413·08	63	+ 839·50	84	+ 671·64
42	+ 623·75	64	= + 824·87	85	= + 722·34
43	= + 864·40				

Tullinbjerget, Christiania, 26th July, 1869.

On the Comparative Clinging of the Limb of Venus to that of the Sun in the Transit of 1874 *as compared with that of* 1882. By R. A. Proctor, B.A.

Although Mr. Stone's paper on the above subject in the April Number of the *Notices*, and my own remarks to the same effect in the March Number, perhaps indicate with sufficient exactness the relation between the two transits in the respect dealt with, it may be worth while to determine the actual intervals which will elapse between real and apparent internal contacts for both transits.

I shall adopt Mr. Stone's estimate that about $\frac{1}{60}$th part of the diameter of *Venus* may be allowed for the effects of irradiation (*Monthly Notices* for November, 1868).

Venus in transit (at her rising node) appears to traverse a space equal to her own diameter in $15^m\ 16^s{\cdot}88$, and therefore $\frac{1}{60}$th of her diameter in $15^s{\cdot}28$. This would be in the interval between real and apparent contacts in the case of a central transit.

Now, in 1874 (see p. 216 of my paper in the March Number of the *Monthly Notices*) the chord traversed by *Venus*, from internal

contact most accelerated at ingress to the same phase most retarded at egress, subtends at the Sun's centre

$$360° - (133° 56' + 163° 27') = 62° 37'.$$

Hence the angle at which *Venus's* apparent path is inclined to the Sun's limb, at the epochs of these phases is 31° 18′; and the interval between real and apparent internal contacts is therefore

$$= 15^s{\cdot}28 \times \operatorname{cosec}(31° 18')$$
$$= 29^s{\cdot}41.$$

In a similar manner the corresponding interval for internal contact most retarded at ingress, and for the same phase most accelerated at egress

$$= 15^s{\cdot}28 \times \operatorname{cosec}(25° 48')$$
$$= 35^s{\cdot}11.$$

The difference, by the way, is greater than one could expect, and shows how necessary it is, in all such cases, to test our views by calculation.

For 1882, I use the position-angles given in a Note in the present Number. I find the interval between real and apparent contacts, for internal contact most accelerated at ingress and the same phase most retarded at egress

$$= 15^s{\cdot}28 \times \operatorname{cosec}(48°{\cdot}3)$$
$$= 20^s{\cdot}46;$$

while for internal contact most retarded at ingress and most accelerated at egress, I find the interval between real and apparent contacts

$$= 15^s{\cdot}28 \times \operatorname{cosec}(45°{\cdot}5)$$
$$= 21^s{\cdot}39.$$

We see, at once, from these values that a given difference of duration will be much more valuable in 1882 than in 1874. Whether an observer in 1874, having from $29^s{\cdot}41$ to $35^s{\cdot}11$ during which to watch the separation of *Venus* from the Sun's limb would make as large a *proportional* error as an observer in 1882 who has an interval of from $20^s{\cdot}46$ to $21^s{\cdot}39$ during which to watch the same phenomenon, may be questioned; that he would make a larger *absolute* error is certain. Assuming the former view we may proceed to compare the time-differences which would have equal values in 1874 and in 1882. Let P be the ratio in which a difference in 1874 must exceed one in 1882, that the two may have equal values; then we must have

$$P^2 = \frac{(29{\cdot}41)^2 + (35{\cdot}11)^2}{(20{\cdot}46)^2 + (21{\cdot}39)^2}$$

(for Delisle's method assumed to be applied to the greatest advantage in either case,—but the ratio is approximately true also for Halley's method). It follows that

$$P = 1{\cdot}547.$$

The ratio I had made use of in the March Number of the *Monthly Notices*, was $50\frac{1}{2} : 32\frac{3}{4}$, which reduces to 1·533, and is appreciably correct; nor does Mr. Stone's value, 1740 : 1219 (or 1·427) in the April Number, differ importantly from the true ratio.* It is worthy of notice, however, how readily a change of the phase considered affects all such proportions. One would have thought that the value which Mr. Stone obtained by considering the intervals between external and internal contact, could not differ appreciably from that which I had obtained by considering the intervals between internal contacts most accelerated and the same phase most retarded by parallax; yet the change is about as from 14 to 13.

Sept. 11th, 1869.

Note on the Transit of Venus in 1882. By R. A. Proctor, B.A.

The elements marked (*) in the following table have been deduced from the formulæ discussed in my paper in the March Number of the *Notices;* the others are Mr. Hind's fundamental elements of the transit:—

	G.M.T. h m s	Position-Angle. From S. to E. °
First External Contact (from Earth's centre)	1 55 38	34·7
*First Internal Contact (most accelerated by parallax)	2 7 51	32·6
„ „ (from Earth's centre)	2 15 56	..
* „ „ (most retarded by parallax)	2 24 18	29·8
		From S. to W.
*Last Internal Contact (most accelerated by parallax)	7 44 5	61·3
„ „ (from Earth's centre)	7 52 27	..
* „ „ (most retarded by parallax)	8 0 32	64·1
Last External Contact (from Earth's centre)	8 12 47	66·1

In the Paper referred to above I expressed my belief "that the corrections which have to be made in the case of the transit

* There is a misprint at p. 221, of the March Number of the *Monthly Notices*, line 13 from the bottom of the page; instead of "observable difference" "maximum difference" should be read. The printer probably took the former reading from the previous sentence. The context shows plainly what was meant; because I immediately apply the maximum differences ($50\frac{1}{2}^m$ in 1874 and $32\frac{3}{4}^m$ in 1882) to the purpose considered. This error, which I have only just detected, serves to explain what had hitherto been quite unintelligible to me, the fact, namely, that Mr. Stone should have written the Paper on the clinging of *Venus* to the Sun's limb without noticing that I had already fully considered the effects of that peculiarity.

of 1882" (when internal contacts are considered instead of passages of *Venus's* centre across the Sun's limb) "are less considerable than those" I had made in the case of the transit of 1874. This view is justified by the calculated positions of the places where ingress and egress are most affected by parallax. These I find to be as follows:—

		Lat.	Long.
(i.)	The place where first internal contact is most accelerated, lies in	51° 5′ S.	86° 48′ E.
(ii.)	The place where first internal contact is most retarded, lies in	53 16 N.	94 28 W.
(iii.)	The place where last internal contact is most accelerated, lies in	26 18 N.	40 1 W.
(iv.)	The place where last internal contact is most retarded, lies in	23 46 S.	137 11 E.

Not one of these places differs by much more than 300 miles from the corresponding place obtained by considering the passage of *Venus's* centre, with the position-angle for external contact,—the phase with reference to which these places had hitherto been calculated.

The transit of 1882 has an interest which that of 1874 wants, in being partially visible in England.

September 11, 1869.

Note on Atmospheric Chromatic Dispersion as affecting Telescopic Observation, and on the Mode of Correcting it. By George Biddell Airy, Astronomer Royal.

In my last communication to the Royal Astronomical Society on the Transits of *Venus*, I adverted to the injurious effect, on the observations, which might possibly arise from the chromatic dispersion produced by the atmosphere. I had carefully observed *Mercury* on the Sun's disk, and I was painfully struck with the colour and consequent indistinctness of the upper and lower limbs both of the Sun and of *Mercury*, and with the prospect of a total loss, in the observations of the Transits of *Venus*, of that delicacy on which the success of the observations will greatly depend: (it being remarked that, for the most valuable observations, the Sun must be low and the atmospheric refraction and dispersion must therefore be considerable; and that the planet must be near the highest or lowest part of the Sun's limb). And I suggested that probably an efficient corrective might be found, in the application of a glass prism of small refracting angle in the eye-piece of the telescope. It is my purpose now to give the numerical elements for the preparation of the necessary prism, and to state the success which attends its use.

The optical theory is exceedingly simple, but it may conduce to clearness that it be given here. Let θ be the angle of incidence when a ray of light falls on a refracting surface; $\theta + \psi$ the angle of refraction; then ψ is the angle of deviation. Also, if the refractive index be $1 + m$, m may be called the index of deviation; m is negative for the passage of light from vacuum to air, and for the passage from air to glass, but positive for the passage from glass to air. At each of these refractions we have the accurate equation,

$$\sin(\theta + \psi) = (1 + m) \sin \theta;$$

and, if θ remain invariable, but m be varied, and ψ be varied in consequence of the variation of m,

$$\cos(\theta + \psi) \times \delta\psi = \sin \theta \times \delta m.$$

Now when ψ is small, but in no other case, $\sin(\theta + \psi)$ is sensibly equal to $\sin \theta + \cos \theta \times \psi$; and in the second equation, the factor of the small quantity $\delta\psi$ may, without sensible error, be considered as $\cos \theta$. Thus, when ψ is small (whatever be the cause of that smallness), the following equations are sensibly accurate:

$$\sin \theta + \cos \theta \times \psi = \sin \theta + m \sin \theta, \quad \text{or } \cos \theta \times \psi = m \sin \theta;$$
$$\cos \theta \times \delta\psi = \sin \theta \times \delta m.$$

From which $\frac{\delta\psi}{\psi} = \frac{\delta m}{m}$; or, the proportion of the change of deviation (accompanying a variation in the colour of the light) to the whole deviation, is the same as the proportion of the change of the index of deviation to the whole index of deviation.

The two deviations which require consideration in the following investigation, namely, that produced by the atmosphere and that produced by a flint prism of small angle, are both small; the first because, though the angle of incidence is large, the index of deviation is very small; the second because, though the index of deviation is large, the angle of incidence is small. The theorem just found will, therefore, apply to both.

In collecting the numerical data to be employed for my calculation I have been referred by my friend Professor W. H. Miller to an account of Ketteler's observations on atmospheric refraction, given in the *Fortschritte der Physik*, 22nd Jahrgang, page 179. For the other media I use some well-known determinations by Fraunhofer. Although, in the practical construction, I use only flint glass, I have thought it interesting to insert the numbers for crown glass and water. The two rays of the spectrum by which I measure the dispersion are B and G.

Medium.	Index of Deviation for B.	Index of deviation for G.
Air ...	0·00029350	0·00029873
Water ...	0·0330977	0·0341261
Crown 13 ...	0·0524312	0·0539908
Flint 30 ...	0·0623570	0·0655406

Now dividing the difference of the indexes of deviation for B and G by half their sum, for each medium, we obtain the following values of $\frac{\delta m \text{ (B to G)}}{m}$:—

Air	...	0·017662,
Water	...	0·030596,
Crown 13	...	0·029310,
Flint 30	...	0·049784.

Air, therefore, is much less dispersive in proportion to its refraction than either of the other media.

I now proceed to apply these numbers to the estimation of the effect of atmospheric dispersion on telescopic vision, and to the construction for its correction.

Let R be the atmospheric refraction of the object viewed; p the power of the telescope. Then the atmospheric dispersion (B to G), as seen by the naked eye, is $R \times 0{\cdot}01766$; and, as seen in the telescope, it is $R \times p \times 0{\cdot}01766$. A prism of flint glass is to be introduced close to the eye, whose dispersion is to neutralize this magnified atmospheric dispersion. Therefore the dispersion of the flint prism (B to G) must be $R \times p \times 0{\cdot}01766$; its refraction must be $R \times p \times 0{\cdot}01766 \times \frac{1}{0{\cdot}04978}$; and, since the index of deviation of the flint glass for the mean rays is 0·6395 (estimated as for the passage from glass to air, which, it is easily seen, give the proportion of the aggregate of deviations at the two surfaces to the refracting-angle of the prism), the refracting angle of the prism must be $\frac{R \times p \times 0{\cdot}01766}{0{\cdot}04978 \times 0{\cdot}6395}$. Performing the numerical calculation, we find that the angle of the flint prism must be $\frac{R \times p}{1{\cdot}8}$, or the atmospheric refraction must be $\frac{1{\cdot}8 \times \text{angle of the flint prism}}{p}$, in order to produce perfect correction.

Suppose, then, that we have a series of flint prisms ground to the angles 2°, 4°, 6°, 8°, 12°, 16°. And suppose that we use a telescope with power 120 or with power 240. Then the following table, showing the zenith distance at which the atmospheric dispersion is corrected, is easily computed; the refraction being calculated by the formula just given, and the zenith-distance corresponding to the refraction being taken from a common table of refractions:—

Angle of Flint Prism.	Telescopic power 120. Atmospheric Refraction.	Zenith Distance.	Telescopic power 240. Atmospheric Refraction.	Zenith Distance.
°	′	° ′	′	° ′
2	1·8	61 58	0·9	43 7
4	3·6	75 16	1·8	61 58
6	5·4	80 15	2·7	70 32
8	7·2	82 52	3·6	75 16
12	10·8	85 34	5·4	80 15
16	14·4	87 3	7·2	82 52

For view with the naked eye it would be necessary to use a prism (of appropriate small angle) with its edge downwards; but, for view with an inverting telescope, the edge of its appropriate prism must be upwards.

Mr. Simms has prepared, under my instruction, a series of prisms of flint glass with the angles just mentioned; and Mr. Carpenter has made trial of them. The following are two of Mr. Carpenter's reports:—

1869, July 15, $11^h\ 50^m$ to $12^h\ 20^m$. *Saturn*, zenith-distance about 82°, viewed with the Great Equatoreal, power 245. Atmospheric dispersion very bad.

Prism 12° corrected the dispersion perfectly.

Prism 8° corrected it fairly.

Prism 6° corrected it partially.

Prism 4° and 2° produced no visible effect.

Prism 16° confused the image.

1869, July 21, 9^h. The Moon, zenith-distance about 78°, viewed with the Altazimuth, power about 100.

Prism 2° slightly diminished the colour.

Prism 4° better than the last.

Prism 6° destroyed the colour entirely. The definition of the details about the Moon's rugged edge was much improved.

Prisms 8°, 12°, 16°, gave an opposite dispersion.

It will be seen here that the object which I proposed to myself is completely attained. It is made possible, by this construction, to examine a celestial body with delicacy and accuracy, under circumstances which would, without this construction, have rendered nice observation impossible.

The series of angles of the prism which I have given appears to me well adapted to general wants. I propose to furnish each of the principal telescopes to be used for the Transit of *Venus* with a complete series of such prisms, arranged perhaps on a long slider. Care must be taken to make the thickness of the slider-frame as small as possible, inasmuch as it must be accompanied with another slider carrying dark glasses. It will probably be

found best to place both sliders between the two glasses of the eye-piece. This slightly disturbs the elements of the calculation above; but in practice the selection of the best prism will always be matter of trial, and the disturbance of calculations will be unimportant.

No assistance is given by this principle to the accurate determination of zenith-distances. The construction, which destroys the prismatic extension of a star's image, necessarily introduces an equal prismatic extension in the opposite direction on the breadth of the wire.

Before closing this subject I will advert to a remark made by one of the most acute telescope-observers who have ever been known in this Society, the late Rev. W. R. Dawes. He states that, in general, a telescope performs better with one particular point of the edge of its object-glass upwards than in any other position. The explanation of this singular remark will be found, I think, in the combination, of the effect of error of centering of the two lenses of an achromatic object-glass, with the effect of atmospheric dispersion. The centre of one lens (using the word "centre" to denote that part in which the tangent-planes of the two surfaces are parallel) ought to be exactly above the centre of the other lens. But it is not easy to make this adjustment perfect; the centre of one lens is frequently above a part of the other lens where the two surfaces have a slight inclination; and the refraction thus created produces in the image of every star a spectrum which rotates as the telescope-tube is made to rotate. In one position of the tube the atmospheric dispersion is opposed to this, and may wholly or in a great measure correct it; in the opposite position the atmospheric dispersion is added to it, and increases its injurious effects.

The atmospheric dispersion between B and G is about $\frac{1}{60}$th of the atmospheric refraction. At zenith-distance 45° it is nearly 1″, at 63° it is nearly 2″, at 80° about 5″. These are the lengths of the visible spectrum.

Royal Observatory, Greenwich,
1869, *August* 7.

Distribution of the Nebulæ. By R. A. Proctor, B.A.

Rather more than two years ago a paper by Mr. Cleveland Abbe on the distribution of the Nebulæ in space was read before this Society. That paper included a valuable contribution to science, in the form of a tabular statement exhibiting the manner in which the irresolvable Nebulæ are distributed over the celestial sphere. A few weeks before I had sent to the editor of the *Student* a paper on the same subject. Until this paper and four others dealing with the distribution of matter throughout that

portion of space which falls within our cognisance had appeaerd, I did not feel at liberty to bring before this Society certain views which have been for a long time in my thoughts. Owing to the press of other matter, and especially of other astronomical matter in the *Student*, the series of papers was only completed last April. This is, therefore, the earliest opportunity I have had of submitting the views I speak of to the judgment of the Royal Astronomical Society.

The four pairs of maps now laid before the Society have been formed from the valuable table prepared by Mr. Abbe; but it is worthy of notice that their teachings differ in no important respect from those conveyed by maps formed from a similar table prepared by Sir John Herschel many years ago. It is important that this should be attended to; because (1) Herschel's table was formed from a catalogue of only 3812 nebulæ, while Mr. Abbe's was found from a catalogue of 5079 nebulæ; (2) Herschel's tabulation corresponded to a division of the sphere into spaces of 1^h in R.A. by 15° in Decl., while Mr. Abbe's took spaces of only 30^m in R.A. and 10° in Decl.; and (3) Herschel included all nebular objects (if I remember rightly) in his tabulation, while Mr. Abbe drew a distinction between the various orders of nebulæ, so that his table as finally drawn up included only 4053 irresolvable nebulæ. When we find that all the refinements thus introduced by Mr. Abbe, and an increase of nearly one-third in the total number of objects dealt with, produce no appreciable change in the indications which the nebulæ present of being aggregated together according to certain definite, and, as I think, very significant laws, we seem free to conclude that no extension of telescopic observation can appreciably affect our views respecting the distribution of the nebulæ. If the second tabulation had indicated laws appreciably different from those which the first had exhibited, the conclusion would have been that we must wait for an increase of knowledge before venturing to theorise; as it is, we may proceed to deal with the facts before us with some confidence as to the general accuracy of their indications. We see, then, that the two tables have an increased value when considered together.

I would first call attention to the pair of maps numbered I. The plan of projection is the isographic projection described in my *Handbook of the Stars.** The meridians and parallels are so drawn as to correspond to Mr. Abbe's tabulation, and in each space as many nebulæ as his table indicates are marked in, so as to spread pretty uniformly over the space. Thus the projection being isographic, the maps represent the actual distribution of the nebulæ (as to density of aggregation, &c.) over the northern and southern hemispheres.

Now when this is done we see that there is much more in the

* After that book was published I found that Sir J. Herschel had already suggested this construction. The coincidence is not very surprising when it is remembered that the problem of a central isographic projection is a very simple one, and has only one solution.



arrangement of the nebulæ than that thinning off in the neighbourhood of the Milky Way which Mr. Abbe had recognised. No one can look at the general aspect of the two maps without seeing that the suggested distribution of the nebulæ in a figure roughly resembling a prolate spheroid, having the plane of the Milky Way at right angles to its longer axis, is wholly insufficient to account for the actual aspect of the maps. The northern map accords better with this view than the southern; but even in this map there is an irregularity in the clustering, an occasional evidence of streaminess, and in places a sharpness in the outline of the group which is altogether different from what would be seen if the nebulæ formed a figure even roughly resembling that suggested by Mr. Abbe. In the southern map all these features are exaggerated. In this map also a new and unexpected feature strikes us. We see that the Nubeculæ are associated by well-marked nebular streams with the main southern group.

But now it is obvious that the pair of maps numbered I. requires to be supplemented by maps which shall exhibit—(1) the two groups of nebulæ in their entirety, and (2) the great zone bare of nebulæ which divides one group from the other.

The pair of maps numbered II. is intended to satisfy condition (1) as nearly as can be done on equatorial maps. The projection corresponds to that used in maps I.* The first map of the pair exhibits the nebular group to the north of the Milky Way, the second exhibits the nebulæ to the south of that zone. It will be noticed that the contrast between the two groups is very striking.

The northern group presents a somewhat stratified aspect, and we can understand how it was that Sir W. Herschel, observing only in northern latitudes, conceived that the nebulæ might be found to be arranged in a zone somewhat resembling the Milky Way, but nearly at right angles to it. On a closer inspection, however, we recognise in the northern cluster a decidedly streamy character.†

* So far as I am aware the pairs of maps numbered II. and III. are the first in which the relations of the nebulæ have been presented isographically on any but a polar projection.

† It is to be noticed that it would be perfectly fair to strengthen the indications of these maps, by spreading the dots which represent the nebulæ less regularly over the spaces they belong to. At first sight this seems illegitimate. It might be argued, for example, that to take the streams presented to us on a first inspection, and to add to their obviousness by a special arrangement of the dots in the figure, is nothing less than to adopt an hypothesis, and to arrange our facts so as to seem to bear it out. But in reality this is not so. The process of arrangement so as to intensify the aggregations of dots and to make the vacuities more clearly apparent corresponds exactly to the process by which, having a series of ordinates in any graphic construction, we take a *curve* through their extremities instead of a series of rectangular cross-lines. The curved lines, in this case, slope upwards towards the direction of the longer ordinates, and downwards towards the direction of the shorter; and precisely in the same way the dots in any of the spaces in such maps as we are now dealing with should aggregate towards the direction in which there are greater aggregations, and be more

The southern group of nebulæ presents features of a very interesting character. As in the northern group, the general aspect of the system is streamy, but this character is much more marked in the southern group. A peculiarity is also noticeable in the fact that the most striking aggregations are at the *extremities* of the streams. In fact, the central part of the whole group, a region exactly opposite the great northern aggregation of nebulæ, is almost vacant. We have, again, distinct evidence of the connexion between the Magellanic Clouds and the southern nebular group. The Nubeculæ are in fact merely instances, more marked than the others, of the tendency to aggregation at the extremities of the nebular streams which form the great southern group.

I now turn to the pair of maps numbered III., which serve to bring out more especially the great vacant zone. This I hold to be the most significant of all the features presented by the nebular system. It will be observed that there is no mistaking the fact that a true zone of nebular dispersion (if I may use that term as the converse of aggregation) exists in the heavens. Nor can the exact agreement of this zone with the galactic zone be called in question. I would now ask whether the conclusions to be drawn from these peculiarities ought not to be somewhat different from those which have been commonly accepted.

In the first place, I would remark that Sir W. Herschel was disposed to look on the position of the zone of nebulæ, which he supposed to exist in the heavens, at right angles, or nearly so, to the Milky Way, as not accidental. The peculiarity we actually have to deal with, however, is much more significant; first, because the agreement of two zones in position is a circumstance which is antecedently much more improbable than mere perpendicularity of intersection; and secondly, because the existence of a zone of dispersion is in itself a much more remarkable circumstance than the existence of a zone of aggregation. On the first point, it is merely necessary to remark that to secure perpendicularity between two zones it is requisite only that the poles of one zone should lie upon the other, which can happen in an infinite number of ways; whereas, to secure coincidence, the poles of one zone must coincide in position with the poles of the other, which can happen only in one way. On the second point, it may be noticed that the existence of a zone of dispersion would be very remarkable even if the zone were not particularly well marked;

sparsely strewn in the direction towards which there are vacuities. Had this been done the streaminess already clearly apparent in the arrangement of the nebulæ would have been rendered much more obvious. In the large diagram* I have felt justified in following this plan. Indeed so far from being unfair, it may be shown to be that which, according to the doctrine of chances, gives the arrangement which would most probably be observed if each nebula had been marked in its proper place.

* This diagram was exhibited at the Meeting.



because all that we know of the universe leads us to look upon systems aggregated into the figures of cylinders, prolate spheroids, or the like, as much less likely to be met with than systems in the figure of disks, rings, oblate spheroids, or the like; and it is only figures of the former sort which can result in showing a zone of dispersion, figures of the latter kind exhibiting (as in the case of the sidereal system) a zone of aggregation. But a very well-marked zone of dispersion, as in the case of the nebular system, is a yet more significant phenomenon, because it can only be explained by looking upon the aggregations between which it lies as distinct systems. Looking on the two nebular clusters in this light, the fact that the Milky Way agrees exactly in position with the zone which separates them, becomes one which we are bound to account for in forming any theory of the nebulæ. In fact, when dealing with the aggregation of nebulæ near the north pole of the galactic system, Sir John Herschel expressed this very opinion; though he has nowhere indicated, so far as I am aware, the conclusion which seems to me to follow directly from that opinion:—

I cannot see how we can look upon the coincidence I have spoken of as *not* accidental, without being led to the conclusion that the nebular and stellar systems are parts of a single scheme. If the nebulæ were external star-galaxies, the coincidence might have happened, but it would be accidental. If they formed a system "distinct from the sidereal system, though involving, and perhaps to a certain extent intermixed with the latter," as Sir John Herschel suggested, the coincidence would still be accidental. If "our system" (again to quote Sir J. Herschel) "lies outside the denser part of the cluster, but is involved within one of its outlying members," or "forms *an element* of some one of its protuberances or branches," yet, again, the coincidence would be accidental. No theory, which looks upon our galaxy as simply a member of the nebular system can possibly be reconciled with the view that the position of the zone of nebular dispersion is otherwise than accidental, unless we assume either (i.) that *all* the members of the system, or at least all in our neighbourhood, have a position parallel to that of our galaxy, or at right angles to the length of the nebular system, or (ii.) that there is some peculiarity in the galactic stratum preventing us from looking so far out into space along its length than elsewhere. The first supposition is disposed of by the briefest study of the nebulæ, which (looked upon as disk-shaped universes) have every variety of position. The second has a greater appearance of probability, yet it would be easy to dispose of it on its own merits. It must be dismissed, however, for a reason associated with a matter which yet remains to be considered.

We have seen that the nebulæ belonging to the Magellanic clouds are associated in a very obvious manner with the rest of the nebular system. But it must not be forgotten that these strange objects are also intimately associated with the sidereal

system. In fact, their visibility to the naked eye is due entirely to the fact that they contain a number of small stars. Here, then, the sidereal and nebular systems are seen as they are nowhere else to be observed, *intermixed.* Perversely enough, this peculiarity, which seems so obviously to indicate an intimate association between the two systems, has been held by many distinguished astronomers, to prove that the Nubeculæ belong neither to the sidereal nor to the nebular system. Sir John Herschel has distinctly pointed out the conclusions which should be drawn from the evidence given by the Nubeculæ; yet he nowhere definitely rejects views to which those conclusions seem directly opposed. He simply remarks that we may be led to look with some suspicion upon views which "in former pages had been somewhat positively insisted upon."*

The evidence afforded by the Nubeculæ seems to me decisive in favour of the intimate association between the stellar and nebular systems, which I have been endeavouring to establish. When it is combined with the evidence before adduced, no reasonable doubt can exist, I think, that the stars and nebulæ are members of the same scheme. There may be individual nebulæ which are true external universes,—It is possible, for example, that the spiral nebulæ may be of this class, as also perhaps the *Andromeda* nebula, and a few others,—but I consider that for one nebula which is really external there are hundreds which are associated with the sidereal system.

What the processes may be which have led to the present arrangement of the great scheme of stars and nebulæ, I shall not venture even to conjecture. I may remark, however, that the great majority of the nebulæ constituting the northern and southern clusters may be looked upon as owing their present constitution to the fact that they are outside the region of most active stellar aggregation, and not to any difference in their original structure. I believe that the irresolvable and scarcely resolvable nebulæ are composed of stars really smaller and really closer than those forming the clusters and easily resolvable nebulæ. In favour of this view I would adduce a peculiarity exhibited in Mr. Cleveland Abbe's tabulation of the nebulæ, the true meaning of which seems to have escaped him.

* I may remark here, in passing, that the tentative and philosophical tone in which Sir John Herschel has reasoned respecting the universe, the light grasp with which he holds the theories he has himself developed, and the wide range over which his views extend have, on the one hand, encouraged me to adopt views opposed to those which he has sanctioned by his approval, and, on the other, to put forward views which he has already mentioned, but to which I have been led by independent considerations. His whole treatment of the subject of the sidereal and nebular systems indicates that the views he has put forward are intended for the most part rather as suggestions than as theories. This power of weighing evidence without reference to preconceived opinions; of indicating the theories suggested by this or that portion of the evidence. and as readily indicating reasons which must lead to the abandonment of such theories, may be looked upon as characteristic of the Herschel family. It would be well for the cause of scientific progress if the power were not so uncommon as it is.



If the objects in Sir J. Herschel's Catalogue are arranged in the following order,—(i.), clusters; (ii.), easily resolvable globular clusters; (iii.), resolvable globular clusters; (iv.), resolvable nebulæ; and (v.), irresolvable nebulæ, and the arrangement of these classes of objects with reference to a galactic zone 10° wide be considered, we find that objects in class (i.) are obviously aggregated on the galactic zone; objects in class (ii.) less so; those in class (iii.) still less so; those in class (iv.) are aggregated away from the galactic zone; and those in class (v.) are aggregated almost exclusively outside that zone. When we take a galactic zone 30° wide, we have,—objects in class (i.) almost exclusively on the galactic zone; those in class (ii.) decidedly aggregated there; those in class (iii.) somewhat less so; while those in the other classes present much the same relations as before.

This plainly shows that the first three of these classes are associated with the galaxy in proportion to their resolvability; the fourth class, which is still resolvable, is removed from the neighbourhood of the galaxy; and the fifth still more so. Now, can we look upon the distinction between the resolvable nebulæ and the resolvable clusters as so marked that, whereas we can admit the latter to be associated with the galaxy (or rather we are forced to do so), we must look upon the former as lying far away beyond the range of the sidereal system? But even if we must do so, how can we then account for the fact that the irresolvable nebulæ show a greater antipathy to the neighbourhood of the galaxy than their resolvable fellow-systems? If we do not, if we look on the resolvable nebulæ as belonging to the sidereal scheme, notwithstanding their aggregation away from the galactic zone, what reason can we assign for forming a different opinion of the irresolvable nebulæ which exhibit precisely the same relation, only in a more marked form? Finally, if we admit the irresolvable nebulæ into the galactic scheme, we cannot look upon greater distance as accounting for their irresolvability, because once they are recognised as associated with the galaxy, we can see no reason for the more distant members of the nebular part of the universe lying towards the poles of the galactic zone. The antecedent probability of such a relation prevailing, that *distance* is the true cause of the irresolvability of the objects forming the great nebular clusters, is exceedingly minute. Of course, *cæteris paribus*, the more distant a cluster is, the less easily resolvable it will be. But we must look for some other cause than distance to account for the fact that nebulæ seen outside the milky way are less easily resolvable than nebulæ seen upon that zone. I believe that cause may be assumed not unreasonably to be the difference in the circumstances under which the galactic and the extra-galactic nebulæ have reached their present state.

I have hitherto omitted all reference to the gaseous nebulæ. So far as present observation goes, these objects show a decided preference for the neighbourhood of the galactic zone. The planetary nebulæ, all of which are probably gaseous, are so distri-

buted, that two-thirds are on the wider galactic zone referred to above, whose area is less than one-fourth that of the whole sphere. The irregular gaseous nebulæ are all on or close to the Milky Way, except one, which is in the greater Magellanic Cloud; another proof, if any were wanting, that the Nubeculæ are not external systems.

In the pair of maps numbered IV. I have combined the visible sidereal system with the nebular scheme; on the same isographic projection, as in maps I. My object was to discover, if possible, whether any laws can be discovered associating the nebular and sidereal systems together. I notice that, outside the Milky Way, the nebulæ seem to be associated with the lucid stars in this way, that, where there are many leading brilliants, there nebulæ seem densest. It is to be noticed that there is not a single marked vacancy in either of the nebular groups where there is not at the same time a marked absence of bright stars. I would point out, also, that, of the two most marked nebular streams, one is coincident with the stellar stream recognised by the ancients in their figuring of the constellation *Eridanus* (the prolongation southwards being recognised by modern astronomers in the constellations *Hydrus* and *Reticulum*); the other coincides with the stellar stream associated by the ancients with the water from the vase of *Aquarius*. This association between stellar and nebular aggregation seems to occupy a character midway between the yet closer relation observed in the Nubeculæ, and the directly contrary relation observed in the neighbourhood of the Milky Way.

On a Method of Determining the Dimensions of the Disks of those Stars which are liable to be Occulted by the Moon. By Richard A. Proctor, B.A.

The following plan for measuring star-disks is theoretically sound, but may probably present great difficulties in practice. It seems worth being tried, however.

If the image of a star is put in rapid rotation (which may be done in many ways) while the centre of rotation moves in a circle, in a period exceeding the maximum duration of luminous impressions on the retina, the appearance presented to the eye will be that of an epicycloidal coil of light, whose rapidly advancing end is always on a black ground. Now if the eye be intently fixed on this coil just before the star's occultation by the Moon, it is obvious that at the instant of occultation the whole coil will vanish; but, during the brief interval which the luminous impression on the retina occupies in vanishing, there will be time to notice the appearance of that end of the coil which was last formed. Now if we suppose the rapidity of rotation such that a quarter of a revolution was made while the Moon was traversing




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the star's disk, it is clear that along that quadrant there will be a gradual diminution of light down to evanescence at the extremity. If the star had an apparent diameter only half as large, the arc of fading light would be but half a quadrant, and so on. The observation would be rendered easier by the fact that a neighbouring part of the coil would always give the means of a ready comparison. I find that if the star's image rotated 100 times in a second, a star ten times as far off as α *Centauri*, and having a real diameter as large as our Sun's, would give an arc of fading light about $55\frac{1}{4}°$ in length. Here I suppose the occultation direct. An oblique occultation would obviously be more favourable.

The light of the star would be much reduced by so rapid a rotation; but with a large telescope I believe quite enough light would be obtained even from a star of the lower magnitudes for the purpose in view. Up to a certain point the reduction of light would be advantageous, as it is much easier to detect differences between faint than between brilliant lights.

An observer would require special training for the use of this method, and experimental observations on terrestrial lights would probably be required before any great reliance could be placed on the resulting measurements of stellar disks.

If reliable measures could be obtained however, the results would obviously be most important. We should learn what relations subsist between the real and the apparent magnitudes of stars, and thus what varieties there may be in the intrinsic brilliancies of these orbs; we should have a ready means of detecting the close duplicity of many apparently single stars; and we could not fail to obtain valuable information on other points of interest. One of the chief ends which the successful application of this method would subserve would be to afford us information as to the possible extinction of light in traversing the inter-sidereal spaces.

As respects this last point I would suggest that a careful comparison of the spectra of faint stars with those of the brighter orbs might indicate the occurrence of extinction; since there is reason for anticipating that if extinction really takes place in space there would be signs of *elective* absorption. If certain portions of the spectra of the smaller stars were found to be relatively fainter than the corresponding portions of the spectra of the brighter stars, or if a tendency to such a relation were noted on the average (for we must not forget that many small stars may be near to us), we should have a strong argument in favour of the theory that light is extinguished in the course of its progress through space.

Sept. 11th, 1869.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1869, *TO JUNE* 1870.

VOL. XXX.

BEING THE ANNUAL HALF-VOLUME OF THE MEMOIRS AND PROCEEDINGS OF THE ROYAL ASTRONOMICAL SOCIETY.

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1870.



MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *November* 12, 1869. No. 1.

WARREN DE LA RUE, Esq., Vice-President, in the Chair.

Lieut. W. H. Collins, R.E.;
Rev. John Edwards; and
Rev. Alphonso Matthey,

were balloted for and duly elected Fellows of the Society.

Solar Eclipse of August 7th, 1869.

By R. T. Paine.

This eclipse was carefully observed by me at the Court House in Boonesboro, Boone County, Iowa, with my small telescope of $2\frac{1}{4}$-in. aperture, power of 30, erect eye-piece, deep green screen. The latitude, $42^{\circ}\ 3'{\cdot}4$, was determined by me, by a few altitudes of *Altair*, which agreed well with each other, but *Polaris* was too faint to be used. The longitude was ascertained by my two chronometers, by comparison with the clock of the Observatory at Chicago, to be $6^h\ 15^m\ 34^s{\cdot}97$; $25^m\ 7^s{\cdot}64$ going to Boone, and $9^s{\cdot}0$ returning from it to Chicago, which is in long. $5^h\ 5^m\ 26^s{\cdot}65$ west from Greenwich. Both latitude and longitude of Boone are, therefore, considered as pretty well known.

The observations were good and the sky clear, although there was some haze.

The corona was good, but, independently of the red or rosy

flames, not, I think, as striking or magnificent as the one at Beaufort, S.C., on November 30th, 1834; moreover, the darkness was *not* as great. I used a lantern on both occasions to read off the chronometers at the second and third contacts, but it was *not* necessary on August 7th. As soon as the second contact took place at Boone the corona was seen, and then a *deep red* flame, a little to the left of the lowest part of the Moon, which remained quite steady to the third; two or three seconds later, two flames of a beautiful rose colour appeared on the left and upper part of the Moon, and continued upwards of a minute: as the Moon moved on, three or four *rosy* flames appeared on the right, which, just before the third contact, seemed to run together. About five minutes before the second, a brush of light descended from the upper crescent of the Sun, and at the *same time* a small part of the lower crescent was broken off, yet nothing of the kind was noticed after the third. The edge of the Moon was generally very sharply defined, but slight irregularities were seen before the second contact, of very short continuance. The red or rosy flames were so conspicuous that they were seen at once, without the assistance of even an opera-glass; but at Beaufort nothing of the kind was noticed, although *α Scorpii*, then (Nov. 30th) in conjunction with the Sun, and therefore only $4\frac{1}{3}^{\circ}$ distant, was seen as soon as the corona. This eclipse will return for the second time on December 22nd, 1870, total at Cadiz, Gibraltar, Syracuse, &c. At Beaufort, lat. 32° 25′ 57″, long. 80° 40′, the interval between the second and third contacts was $1^{h}\ 49^{s}.6$, and the eclipse was quite central.

The total eclipse of June 16th, 1806, which occurred *here*, central, about half-an-hour before noon, was the finest in the United States in the nineteenth century. The duration of totality at Boston and at our neighbouring city, Salem (where it was observed with great care by the late Dr. Bowditch), was *five* minutes. The excitement about it was great indeed, yet no one saw any red flames, and Dr. Bowditch never gave the least hint that he saw any; and a very intelligent gentleman of Lynnfield, who saw that eclipse in that town, and distinctly recollects the phenomena attending it, and the last eclipse at Springfield, Illinois, tells me he is positive the darkness in June 1806 was much deeper, and that there were no flames, as he certainly must have seen them, as he easily did at Springfield. Yet at the second and third returns of the eclipse of 1806, in 1842, and 1860, these flames were generally observed. The fourth return on July 29th, 1878, will be central in the United States, in Colorado, Texas, &c., and will therefore, doubtless, be carefully observed by even a greater number than that of August last, although then the number of observers on the central line, or very near it, was not small.

The phases of the eclipse of August 7th, 1869, at Boonesboro, Iowa, lat. 42° 3′ 23″, long. 93° 53′ 45″, were as follows, by strict computation and observation: —



	By computation. h m s	By observation (Mean of two Chrons.) h m s
Beginning of Eclipse	3 40 37·6 M.T.	3 40 46·78
,, Totality	4 43 20·6	4 43 24·48 = +3·88
End of Totality	46 20·6	46 23·28 = +2·68
,, Eclipse	5 43 36·5	5 43 30·18
Duration of Totality	3 0·0	2 58·8

Boston. Oct. 2nd, 1869.

Solar Eclipse of August 7th, 1869. By E. D. Ashe, Commander R.N.

(*Extract from a Letter to Mr. De La Rue, dated Observatory, Quebec, August* 28, 1869.)

I hasten to communicate to you the result of the Canadian eclipse party that went to Jefferson, Iowa. It consisted of Mr. Douglas, Mr. Falconer, and myself. The funds would only admit of my taking the equatoreal of 8-inch aperture, 9 feet focus, therefore I leave to the American astronomers the description of the results obtained by the spectroscope, &c.

Jefferson is a little to the south of the central line of eclipse. The totality was a few seconds over three minutes. The morning was cloudy; about noon the Sun appeared, and although there were no clouds during the eclipse, still it was hazy.

The total eclipse began at $4^h\ 41^m\ 6^s$, so the Sun was at a very good altitude.

I took several photographs of the partial eclipse, enlarged by an eye-piece to $3\frac{1}{4}$ inches; one shows distinctly a band of light round the limit of the Moon on the Sun. I was afraid of the hazy atmosphere, and altered my arrangements for taking photographs during totality with the eye-piece, and put in the tube fitted for taking the Sun in the principal focus; and had I not done so, the actinic power was so bad that I should have failed completely. In consequence of the change I had no wires to connect the protuberances with the axis of the Sun; but this is of no consequence, as the American party fifty miles to the southward have well determined their position.

I took four negatives* during totality with an exposure of 10^s, and when they are examined with a magnifying glass they are full of information.

* These are now in Mr. De La Rue's care.—Ed.

American Photographs of Total Solar Eclipse of August 7, 1869.

By Rev. T. W. Webb.

Through the courteous intervention of my valued correspondent W. S. Gilman, Jun. Esq. of New York (himself an accomplished observer of astronomical phenomena), I have been favoured by Professor Mayer, of Lehigh University, Pennsylvania, with some specimens of the photographs taken by him during the late total Solar Eclipse of August 7. As these were sent direct from America to Somerset House, I have not had an opportunity of seeing them; but, as I have every reason to believe that they will prove acceptable to the Society, I beg permission to forward to you the following explanatory statement, taken, with some abridgment, from Professor Mayer's letter to myself:—

"The Photographic Expedition, of which these photographs are some of the results, was organized by Prof. Henry Morton, Secretary of the Franklin Institute, Philadelphia, under the authority of Prof. I. H. C. Coffin, U.S.N., the Superintendent of our National Nautical Almanac. The expedition was divided into three parties, stationed respectively at Burlington, Mount Pleasant, and Ottumwa, in the State of Iowa. To me was assigned the organization and command of the station of Burlington, and the photographs I send were taken by me at that place.

Burlington, Iowa, is a town on the W. bank of the Mississippi River, situate in Lat. N. 40° 48′ 21″·58, Long. $0^h\ 56^m\ 13^s{\cdot}88$ W. of the Observatory at Washington. This station was about 7 miles N. of the centre of the Moon's shadow.

"The telescope, by Merz and Mähler, of 6·42 inches aperture and 9 feet focus, was equatorially mounted, and driven by one of Frauenhofer's friction-governor clocks. The Sun's image, 2·04 inches in diameter, was formed on the plate of the camera by a negative eye-piece specially calculated to give the least aberration. The image of a reticule of two spider-threads at right angles was also projected on the plate with the Sun's disk, and one of these lines was accurately adjusted to the celestial equator; and thus the photographs give precise position-angles of the contacts and of the protuberances.

"A plate having a slot of $\frac{1}{10}$ inch in breadth shot across the eye-piece by the action of a spring for the exposure during partial phase. The duration of this *flash* of the Sun upon the camera-plate I have made the subject of experimental investigation; and I find it to have been almost exactly $\frac{1}{500}$th of a second. A 2-inch aperture of object-glass was used during partial phase work. During totality the full aperture was used, and a slide which allowed the whole beam to fall upon the plate; the exposures varying from 5 to 7 seconds.

"Forty-one perfect photographs were taken during the eclipse, and five of these (all of which I send) were taken during totality, which lasted with us $2^m\ 42^s$. I send nine plates taken at the

times I place opposite the numbers, which correspond with those on the plates.

	No.		Sidereal Time of Burlington. h m s
Before Totality	4	.. ..	13 2 4·1
	19	.. ..	13 49 39·8
	21	.. ..	13 58 32·4
Totality	23 (Exposure 5ˢ)		14 4 1·4
	24 („ 5)		14 4 29·6
	25 („ 7)		14 4 52·8
	26 („ 7)		14 5 10·37
	27 („ 7)		14 5 40·5
After Totality	35	.. ..	14 34 59·5

"The times were electrically recorded on a chronograph by the exposing plate breaking the electric circuit.

"The photograph numbered 4, taken 2ˢ·8 after *observed* contact, shows a depression in the Sun's limb at the position of first contact, and from this depression shoots into the Sun a high lunar mountain, whose position, measured from the S. point of the cusps, is ¼ of the distance to the N. point of the same.

"You will observe how beautifully defined are the two large spots in the S.W. and N.E. quadrants; the latter surrounded by well-developed faculæ, one of which seems to bridge over the spot and divide it into two portions. I also call attention to the gradation of shade from the border of the Sun inward, the faculæ, the mountains on the Moon's limb, and a glow like that of early dawn (also obtained in the photographs by Mr. De La Rue in 1860), which extends to 18″ beyond the limb of the Moon.

"I could hardly have wished for better success during totality. I might probably have obtained more of the corona by longer exposures, but it would have been at the expense of the fine definition in the prominences, to which my work was specially directed.

"Will you have the courtesy, my dear Sir, to present to the Astronomical Society the nine photographs on glass taken from the original negatives with an orthoscopic lens: I wish them to be preserved in their collection. The other three enlarged copies on paper (also made with an orthoscopic lens) I wish you to receive as a present from me. The best way to examine the glass photographs is to incline them over a piece of white drawing paper placed before a north window, and use a lens magnifying about eight diameters."

Thus far, in substance, Professor Mayer, who has also kindly offered for the Society's acceptance a copy of his Report on the Eclipse, published in the Journal of the Franklin Institute, which contains many additional particulars of much interest. He has also presented to them a copy of his Lecture-Notes on Physics.

Hardwick Vicarage, Nov. 10, 1869.

The November Star-Shower in 1869. By A. S. Herschel, Esq.

The observations of the appearances of the November Meteors, during the last few years of their great returns, present certain irregularities, which it is either possible for physical astronomy to explain, by calculating the perturbations exercised upon the course of the stream by the attraction of the planets, or which may arise from the peculiar configuration of the meteoric current. Supposing the main body of the November meteors, which, in the years 1866 and 1867, made its appearance, successively, in Europe and America, to be an individual portion of the stream, subject during the years 1868 and 1869 to a continuance of the same perturbations which made its return in the year 1867 take place two hours later than the time when (from its occurrence in 1866) it was expected to appear; supposing also that the density of the current, at the point where the Earth will traverse it in the present month, is not yet exhausted,—the time of reappearance of the main-stream of meteors, this year, in England will be almost exactly the same,—between 1^h and 2^h A.M. on the morning of the 14th instant,—as that of its greatest intensity in 1866. A watch for meteors continued from the time of *Leo's* rising on Saturday night the 13th, until sunrise on Sunday morning next, the 14th instant, will probably be rewarded by a view of the return of the central portion of the meteor-stream in Great Britain, although, doubtless, owing to the waning character of the phenomenon, with diminished intensity, yet under the same favourable conditions as those in which it was observed in England in 1866.

Observations at other times and places than those of the great returns of the star-shower in 1866 and 1867 appear to indicate the existence of two meteoric currents bordering upon the main-stream, but separated from it by blank spaces almost entirely devoid of meteors, and forming lateral outliers of the stream, near to which they appear to move in parallel, and closely adjacent orbits. The passage of the Earth through the first of these outlying streams occurs about twelve hours earlier, and its passage through the last about twelve or fifteen hours later, than its appulse with the main or central current. For a knowledge of this peculiar conformation of the November meteoric stream, as exhibited in its occurrences of the last few years, I am indebted to a very important observation communicated to me at the beginning of the present year by Mr. Benjamin V. Marsh, of Philadelphia, U.S., of the occurrence of a remarkably brilliant meteoric shower in China, on the morning of the 15th of November, 1867, *about twelve hours later, in absolute time*, than the beginning and end of the great display seen *in the same year* in the United States.* The times of the previously recorded star-showers, and of that conspicuously seen in America last year,

* *Proceedings of the American Philosophical Society*, vol. x. p. 384.

are thus graphically exhibited by Mr. Marsh in a single diagram (No. 1), which shows the dates and hours of their occurrence, in Philadelphia time, reduced to the common epoch of the year 1868.

Diagram No. 1.

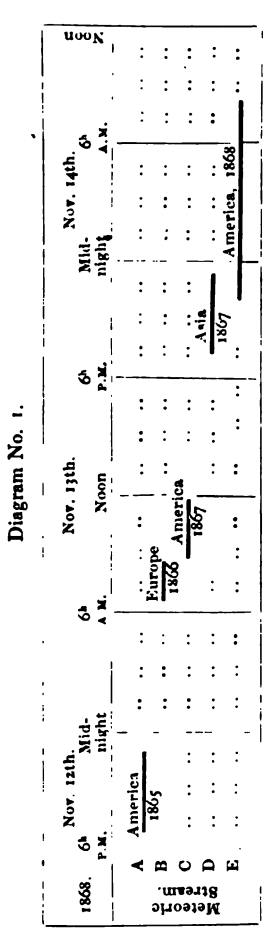
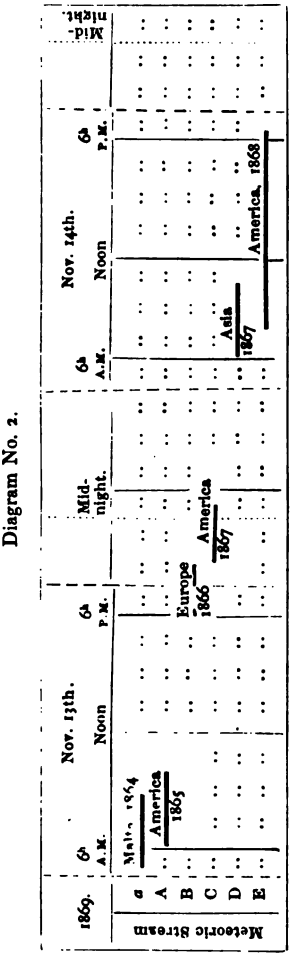


Diagram No. 2.



	Place, Year, and Local Time of Maximum.					Philadelphia Time of Ditto.				
			h	m				h	m	
Shower A,	Philadelphia, 1865,	Nov. 13,	3	0	A.M.	1865, Nov.	13,	3	0	A.M.
B,	London, 1866,	,, 14,	1	15	,,	1866, ,,	13,	3	0	P.M.
C,	Philadelphia, 1867,	,, 14,	4	30	,,	1867, ,,	14,	4	30	A.M.
D,	Shanghai, 67,	,, 15,	4	30	,,	67, ,,	14,	3	30	P.M.
E,	Philadelphia, 1868,	,, 14,	3	0	,,	1868, ,,	14,	3	0	A.M.

Philadelphia should have crossed those streams in 1868 (as shown in the diagram), at the following times:—

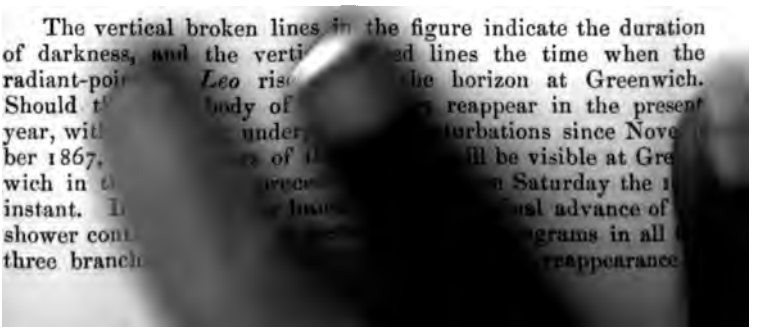
	Meteoric Stream and its Duration. (Philadelphia Time).		Maximum. (Philadelphia Time).
	h m	h m	h m
A,	1868, Nov. 12, 7 0 P.M.	to 11 0 P.M.	1868, Nov. 12, 9 0 P.M.
B,	,, ,, 13, 7 0 A.M.	,, 9 0 A.M.	,, ,, 13, 8 0 A.M.
C,	,, ,, 13, 9 0	,, ,, 12 0 Noon	,, ,, 13, 10 30 ,,
D,	,, ,, 13, 7 30 P.M.	,, 11 30 P.M.	,, ,, 13, 9 30 P.M.
E,	,, ,, 13, 10 0 P.M.	,, 8 0 A.M. (Nov. 14)	,, ,, 14, 3 0 A.M.

A simple inspection of the diagram will suffice to show that the great shower of meteors seen last year in Europe and America corresponds more nearly, in the time of its appearance, *to the Asiatic star-shower*, than to the American apparition of the meteors in 1867; while the latter appears to coincide most nearly with a reappearance of the main body of the meteors observed in Europe in the previous year.

By adding five hours to the times in the last of the foregoing tables, for the longitude of Philadelphia west from Greenwich, and six hours more for the fraction of a day in one tropical year elapsed since the epoch for which it was prepared, the following diagram (No. 2, p. 7) is easily drawn from that obtained by Mr. Marsh, to aid observers of the shower in England in the present year:—

The shower (*a*) is added to the list from an observation recorded in the "British Association Report for 1865," p. 122; and the times when Greenwich should cross the several streams this year are as follows:—

	Meteoric Stream and its Duration. (Greenwich Mean Time).		Maximum. (Greenwich Mean Time).
	h m	h m	h m
a,	1869, Nov. 13, 5 0 A.M.	to 9 0 A.M.	1869, Nov. 13, 7 0 A.M.
A,	,, ,, 13, 6 0 ,,	,, 10 0 ,,	,, ,, 13, 8 0 ,,
B,	,, ,, 13, 6 0 P.M.	,, 8 0 P.M.	,, ,, 13, 7 0 P.M.
C,	,, ,, 13, 8 0 ,,	,, 11 0 ,,	,, ,, 13, 9 30 ,,
D,	,, ,, 14, 6 30 A.M.	,, 10 30 A.M.	,, ,, 14, 8 30 A.M.
E,	,, ,, 14, 9 0 A.M.	,, 7 0 P.M.	,, ,, 14, 2 0 P.M.



The vertical broken lines in the figure indicate the duration of darkness, and the verti[illegible]d lines the time when the radiant-poi[illegible] *Leo* ris[illegible]e horizon at Greenwich. Should t[illegible]body of [illegible] reappear in the present year, wit[illegible] under[illegible]turbations since Nove[illegible]ber 1867, [illegible] of t[illegible]ll be visible at Gre[illegible]wich in t[illegible] Saturday the 1[illegible] instant. I[illegible] [illegible]al advance of [illegible] shower con[illegible] [illegible]grams in all [illegible] three branch[illegible] [illegible]reappearance

the central shower will commence at Greenwich on Saturday at midnight, and continue until Sunday morning, at 3^h A.M.; the circumstances in this case being as favourable for its complete observation as they were on the night of the 13th–14th of November, 1866.

Although it does not appear that the outlying currents of the group are this year favourably situated for observation in Great Britain, yet the observation of a frequency of meteors towards daybreak on the morning of the 13th (Saturday) and of an unusual number of meteors after 11 o'clock P.M. on the night of the 14th (Sunday) would confirm the probable existence of two companion streams of the central meteor-current, and would afford material data for determining the limits of their extent.

The large meteor which was very generally seen in the south of England at about $6^h\ 50^m$ P.M. on the evening of the 6th instant, was also seen at Hawkhurst, in Kent, and the position of the luminous streak which it left visible, standing, apparently, quite perpendicularly over the point where the meteor disappeared, was noted exactly by the stars. It extended from *Vega Lyræ* to a point two or three degrees above and to the right of the star *μ Herculis*, where the meteor burst into fragments of white blue and orange colours, and left a small cloud of light at that spot, like a nebula, which remained visible for several minutes. The observer particularly noticed the extreme rapidity of the meteor's flight, and its perpendicular direction towards the Earth; but no sound of a report was heard, although he listened attentively for some minutes. The direction of the meteor was nearly due west, and its point of disappearance was about 40° from the horizon.

Note on the Sun's Motion in Space and on the relative Distances of the Fixed Stars of various Magnitudes. By Richard A. Proctor, B.A.

Having recently had occasion to examine Mr. Main's Table of the Proper Motions of 1167 Stars, and the conclusions with reference to the Sun's motion deduced from that table by a method devised by the Astronomer Royal, and carried out at his request by Mr. Dunkin, I have been led to notice certain facts which seem to me to be not without significance.

In the first place, I would call attention to the table drawn up by Mr. Dunkin, in which the sums of the squares of the stars' proper motions are compared with the corresponding sums when the proper motion of each star has been corrected for the Sun's calculated motion in space. Mr. Dunkin comments on the singular smallness of the correction thus introduced. And this view is abundantly verified by the table, which runs as follows, divisions 1 to 7 corresponding to Struve's arrangement of the order of magnitude:—

Sums of Squares of Motion in Parallel.

	Uncorrected.	Corrected.
Division 1	2·0637	1·2123
2	1·8743	1·9292
3	9·2894	9·5607
4	6·4732	5·4608
5	43·4126	42·4236
6	14·8637	14·2750
7	0·7814	0·7215
Sum	78·7583	75·5831

ums of Squares of Motion in N.P.D.

	Uncorrected.	Corrected.
Division 1	6·7231	5·6883
2	0·5351	0·7805
3	4·9739	4·7569
4	6·9390	6·5255
5	39·4335	38·7292
6	4·5671	4·3376
7	0·0951	0·0904
Sum	63·2668	60·9084

Commenting on this result, Sir John Herschel remarks:—"No one need be surprised at this. If the Sun move in space, why not also the stars? And if so, it would be manifestly absurd to expect that any movement could be assigned to the Sun by any system of calculation which would account for more than a very small portion of the totality of the observed displacements."

It had always seemed to me that this conclusion might require to be modified if the question were subjected to mathematical scrutiny; my reason for forming this view being this,—that the largeness of the number of stars operates as much to increase the extent of the correction as to increase the amount of the uncorrected squares, since every star is affected by the Sun's motion in space.

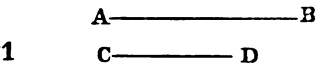
It occurred to me recently that the following simple geometrical proof serves to show that the correction to be looked for is much larger than that which Mr. Dunkin's figures exhibit.

Suppose that in any small region of the sky there are a large number of stars (say n), all at the same distance from the Earth, and travelling in all directions with a velocity exactly equal to that with which the Sun is travelling. Let us suppose that the motion of any one of these stars which is travelling at right angles to the line of sight carries the star over an arc p in a year; and that the region of the sky is so situated that the effect of the

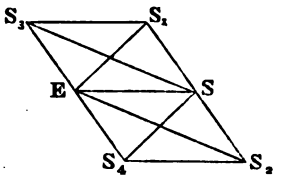
Sun's motion on a star at rest (at the given distance) would be to produce an apparent annual proper motion q'.

Let A B (fig. 1) $= p$
C D $= q'$

Fig1



Suppose two of the stars to move in opposite directions, $S S_1$, $S S_2$, along the same straight line, both (we may suppose for convenience) starting from S, and the lines $S S_1$, $S S_2$, being in reality equal to A B, but foreshortened. Then the sum of squares for these stars, if unaffected by the Sun's motion, would be



$$= S S_1^2 + S S_2^2,$$

where $S S_1$, $S S_2$, represent the *apparent* lengths of these lines.

Suppose that S E is the apparent motion due to the Sun's real motion, so that S E = C D.

Complete the parallelograms $S S_1 S_3 E$ and $S E S_4 S_2$, and draw their diagonals. Then the sum of squares for the two stars as affected by the Sun's motion,

$$= S S_3^2 + S S_4^2$$
$$= S_2 E^2 + S_1 E^2$$
$$= S S_1^2 + S S_2^2 + 2 E S^2$$

Therefore, for every pair of stars moving in opposite directions, there is an increase of $2 q'^2$ in the sum of squares. Therefore there is an average increase of q'^2 for each star in the region.

Now a moment's consideration will show that, precisely as the lines $S S_1$ and $S S_2$ vary from the full length A B to zero, as we vary the supposed motion in all directions around S (for stars in a given small region of the heavens), so does C D vary from A B to zero as we shift the small region over the heavens. It is not merely that the limits of change are the same, but the proportion of lines of a given length $S S_1$ for the star's motion is exactly the same as the proportion of corrections equal to $S S_1$ for the Sun's motion. Since, therefore, for each region there is an increase per star equal to the square of the proper motion due to the Sun's motion (for that region), and the several stars of each region have proper motions varying according to exactly the same law as that according to which the effect of the Sun's motion varies over the celestial sphere, it is obvious that for the whole sphere the effect of the Sun's motion must be to increase the sum of squares by an amount exactly equal to the sum of squares due to the stars' own motions. In other words, the sum of squares is doubled

through the effect of the Sun's motion, if only the Sun be assumed to travel at the same rate through space as the several stars at a given distance from him.

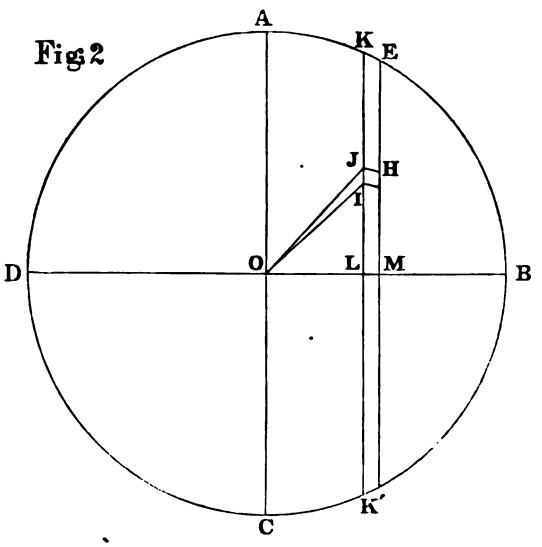
It is obvious also that the same is true if the stars at a given distance move with different velocities, but the Sun's velocity is the mean (its square equal to the mean of sum of squares) of the velocities of the stars at said distance.

Further, if the mean velocity of the stars at a given distance be p, and the Sun's velocity be q, then we have the ratio

$$\frac{\text{Corrected sum of squares}}{\text{Uncorrected sum}} = \frac{p^2}{p^2 + q^2}.$$

Before proceeding to apply this law, I propose to deduce it by another process, which will serve to indicate what is the actual sum of squares, corrected or uncorrected, for a given large number of stars, all at the same distance from the Earth.

Fig 2



Let A B C D, fig. 2, represent a small circular space on the celestial sphere; and suppose a large number of stars within this space, travelling in all directions with a mean velocity which would give to one moving at right angles to the line of sight an annual proper motion $= p$. Then, in order to determine the mean square of the apparent proper motion — the Earth being supposed to be at rest — we may suppose every star to start from O, O B $= p$, and the points towards which the stars are severally moving, uniformly spread over the sphere A B C D.

Take now a thin zone of the sphere's surface by parallel planes perpendicular to O B, through L K, M E, where

$$\text{O L} = x, \qquad \text{O M} = x + \delta x.$$

Then the number of stars whose directions lie towards some part of this zone, is (by a well-known property of the sphere),

$$= \frac{n \cdot \delta x}{2p},$$

n being the total number of stars.

Again, take planes B O I, B O J, inclined at angles θ and $(\theta + \delta\theta)$ to the plane through O B and the observer's eye.

Then the number of stars, whose direction is towards some part of the small element I H of the sphere's surface,

$$= \frac{n\,\delta x}{2p} \cdot \frac{\delta\theta}{2\pi}.$$

When the element I H is taken small enough, each of these stars has an apparent proper motion, whose square

$$= x^2 + (p^2 - x^2)\sin^2\theta.$$

Therefore, it follows that the mean value required

$$= \frac{1}{p\pi} \int_0^{\frac{\pi}{2}} \int_{-p}^{+p} \left\{ x^2 + (p^2 - x^2)\sin^2\theta \right\} d\theta\, dx$$

$$= \frac{2p^2}{\pi} \int_0^{\frac{\pi}{2}} \left(\frac{\cos^2\theta}{3} + \sin^2\theta \right) d\theta$$

$$= \frac{2p^2}{3\pi} \int_0^{\frac{\pi}{2}} \left\{ \frac{1 + \cos 2\theta}{2} + \frac{3\,(1 - \cos 2\theta)}{2} \right\} \delta\theta$$

$$= \frac{2p^2}{3}; \qquad (1)$$

and the sum of squares is therefore

$$= \frac{2np^2}{3}. \qquad (2)$$

Next let us inquire what the sum would be if the solar system were moving in such a manner that each star within the circle A B C D was affected by a proper motion q (in any direction) due to the Sun's motion.

Let the motion be parallel to O B, and from right to left. Then it is clear that the only change in the expression to be integrated, is, that for x^2 we must write $(x - q)^2$. Therefore the mean square will be

$$\frac{2p^2}{3} + \frac{1}{p\pi}\int_0^{\frac{\pi}{2}}\int_{-p}^{+p} (q^2 - 2qx)\, d\theta\, dx$$

$$= \frac{2p^2}{3} + q^2. \qquad (3)$$

In this case, then, the sum of squares is

$$= \frac{2np^2}{3} + nq^2. \qquad (4)$$

Lastly, we have to inquire what is the sum of squares for N stars scattered over the whole of the celestial sphere at a given distance R, each affected by the proper motion p *in space*, the Sun being affected by a motion P (p and P representing the arc-motions due to these respective proper motions, when supposed to be taking place in a direction at right angles to the line of sight, and to be seen from a distance R).

In fig. 2, let A B C D now represent the celestial sphere, and take

$$O L = y$$
$$L M = \delta y;$$

then the effect of the Sun's motion upon all stars in the zone KMK′ will be to affect them with a proper motion,

$$P \sin \cos^{-1}\left(\frac{y}{R}\right)$$

$$= P\sqrt{\left(1 - \frac{y^2}{R^2}\right)}.$$

Hence, by (2) the average proper motion of stars in this band

$$= \frac{2p^2}{3} + P^2\left(1 - \frac{y^2}{R^2}\right);$$

and, therefore, for the whole sphere, the average proper motion,

$$= \frac{1}{2R}\int_{-R}^{+R} \left\{\frac{2p^2}{3} + P^2\left(1 - \frac{y^2}{R^2}\right)\right\} dy$$

$$= \frac{2p^2}{3} + P^2 - \frac{P^2}{3}$$

$$= \frac{2}{3}(p^2 + P^2). \qquad (5)$$

and the sum of squares of proper motions

$$= \frac{2N}{3}(p^2 + P^2) \qquad (6)$$

If $P = p$ (that is, if the Sun's motion be assumed equal to the average motions of the stars) the sum of squares,

$$= \frac{4N}{3} \cdot p^2. \qquad (7)$$

The use of the integral calculus, as above, would only be justified where N is infinite; but where N is considerable, the result must be a close approximation to the truth, with the assumed conditions. And even where N is small, the above result is the most probable, in the case of a random distribution of the N stars, and of the directions of their motion.

Now, if we apply these results to the tables given above, we can determine how far the observed proper motions of the stars are consistent with the supposition that the Sun's proper motion is not very different from the mean proper motion of the stars of different magnitudes; and thence we can form an opinion as to the justice of those estimates of the stars' distances on which the values of the corrections tabulated above have been determined.

First, we require the sums of squares of proper motions, without reference to direction. Since the square of a star's proper motion is equal to the sum of the squares of the star's proper motion in parallel and in N.P.D., we have only to add the respective sums of squares in the two tables given above, to deduce the following table:—

Sums of Squares of full Proper Motion.

	Uncorrected.	Corrected.	N.
Division 1	8·7868	6·9006	9
2	2·4094	2·7097	55
3	14·2633	14·3178	146
4	13·4122	11·9863	238
5	82·8461	81·1528	330
6	19·4308	18·6126	368
7	0·8765	0·8119	21
Sum	142·0251	136·4917	1167

Let us first apply formulæ (2) and (6), (remembering that formula (2) is true for the whole celestial sphere). We have from them,—

$$\text{Uncorrected sum of squares} = \frac{2N}{3}(p^2 + P^2),$$

$$\text{Corrected sum of squares} = \frac{2N \cdot p^2}{3}.$$



Hence

$$P^2 = p^2 \left(\frac{\text{correction}}{\text{corrected sum}}\right).$$

Applying this successively to the several divisions, we obtain

For Division 1	$P = p$ (·52)
2	P has an imaginary value
3	P has an imaginary value
4	$P = p$ (·34)
5	$P = p$ (·18)
6	$P = p$ (·21)
7	$P = p$ (·28)

The results for the second and third divisions suffice to show that the numbers of stars which fall under these heads are insufficient for a satisfactory determination; and therefore, *à fortiori*, the number of stars in division 1 is insufficient. Hence the result $P = p$ (·52) must be dismissed as valueless. But even at this stage of the inquiry we begin to recognise that there must be something wrong about our assumptions. For even if the numbers 55 and 146 were not in themselves large enough to lead us to expect a satisfactory evaluation for stars in the second and third divisions, yet the fact that the Sun's motion in space has been correctly deduced from a smaller number of stars would justify such an expectation.

I should be led then to suspect from this evidence, that if divisions 1, 2, and 3, had been taken together, a more satisfactory conclusion would have been arrived at, notwithstanding the apparent necessity of assigning different distances to stars in these divisions. However, it is not possible to determine how far this suspicion is justified without going over Mr. Dunkin's labours afresh, with changed assumptions; and I have no leisure for the long processes of calculation this would involve.

Next we may notice that the results for divisions 4, 5, 6, and 7, are very far from satisfactory. I cannot think it credible that the real solution of the difficulty involved in the smallness of the observed corrections is to be found in the assumption that the mean motion of stars of the fourth magnitude is three times as great as the Sun's, the mean motion of fifth-magnitude stars nearly six times as great as the Sun's, and so on.

If we try the effect of diminishing the assumed distances of stars of the 4th, 5th, and 6th divisions, so as to accord with the observed relations, on the assumption that in reality $P = p$, we have (the assumed distances being 3·76, 5·44, and 7·86,* respectively) the following results:—

* Struve's values.

For Division 4	Distance = 3·76 × ·34 = 1·28
5	,, = 5·44 × ·18 = 0·98
6	,, = 7·86 × ·21 = 1·65

As division 7 includes but 21 stars, we cannot expect any trustworthy results from treating it in the same way.

The evidence thus far seems strongly opposed to accepted views respecting the distances of stars of the smaller visible magnitudes. A further inquiry on this point seems, therefore, suggested. And it is obvious that in the above table of sums of squares we have the means of testing how far the assumed distances of the stars of various orders accord with the observed proper motions. I make the assumption that the Sun's motion is equal to the average proper motion of the fixed stars. This assumption affects the actual, but not the relative, values of the distances which result from the following processes.

We have then formula (7) to deal with. Applying it to the successive orders of stars, *i.e.* putting $\frac{4}{3} N p^2$ successively equal to the values tabulated in the foregoing columns of uncorrected sums of squares, we obtain,

	Apparent Proper Motion.	Resulting Distance.
Division 1	0·857	1
2	0·182	4·7
3	0·268	3·2
4	0·208	4·1
5	0·433	2·0
6	0·191	4·5
7	0·173	5·0

This result is unsatisfactory in the extreme. We find stars of the second magnitude placed (according to this mode of estimating their distances) further from us than stars of the 3rd, 4th, 5th, and 6th magnitudes.

Remembering the evidence we have already had, that (1) there is something erroneous in our assumptions respecting star-distances; and secondly, that small numbers of stars are insufficient for our guidance, let us apply a test which there ought to be no mistaking. Let us divide the stars into two sets, the first including divisions 1, 2, 3; the second, the remaining divisions and let us apply formula (7) to these sets.

We obtain,

For Set 1, Apparent Proper Motion,

$$= \sqrt{\frac{3}{4}\left(\frac{25{\cdot}4595}{210}\right)} = 0''{\cdot}3015,$$

B



For Set 2, Apparent Proper Motion

$$= \sqrt{\frac{3}{4}\left(\frac{116{\cdot}5656}{957}\right)} = 0''{\cdot}3022.$$

This result would make the mean distance of stars of the first three magnitudes equal to (or *very slightly less* than) the mean distance of stars of the next three magnitudes!

I am very far from supposing that this result accurately represents the relations subsisting among the stars; but I do think that it suffices to render the usually accepted views respecting stellar distribution wholly untenable. Remembering that whatever theory we form regarding the relation between the apparent brilliancy and the real distance of the stars, we must yet recognise the fact that the stars are at very various distances from us. I think it must be admitted that the apparent brightness of a star is, to a certain extent, an argument of relative proximity. A large proper motion is also an argument of relative proximity. If the two indications agreed either for separate stars or for sets of stars, arranged according to apparent brilliancy, there would be no difficulty. As we find, however, that there is no such agreement, we are forced to consider whether brightness or large apparent motion is the stronger evidence of proximity. Judging from the analogy of the solar system, in which the range in the variations of magnitude is enormously greater than the range in the variations of velocity, we seem strongly led to look on the proper motions of stars as in reality the best evidence we have respecting their distances. But this conclusion is very much strengthened when we remember that the dynamical conditions in the sidereal system must be much more unfavourable to the occurrence of wide variations of velocity, than the conditions which prevail in a system of bodies circling around a central body enormously large compared with any of its dependent orbs.

I think, then, that I may fairly look upon the above inquiry as affording very striking evidence in favour of the view I had formed from other considerations, that the assumed estimate of the distances of the smaller stars has been greatly overrated. And as this conclusion may obviously be extended to yet smaller stars, I think that I have not been deceived in looking upon the relations which subsist between the Milky Way and the lucid stars in its neighbourhood as very much more intimate than has been commonly supposed. I believe that future researches will prove, not only that the Milky Way as a whole is much nearer than we have been imagining, but that portions of it are absolutely nearer to us than the brightest of the single stars. That parts of the Milky Way, for instance, in the neighbourhood of *α Centauri* (the nearest of the fixed stars, so far as is yet known), are nearer to us than that star, I think the whole aspect of the galaxy in that neighbourhood suffices to suggest, if not to demonstrate.

Suggestion of a Method of Imitating the Transit of a Planet over the Sun. By the Rev. T. R. Robinson, D.D., F.R.A.S., &c.

While reading Mr. Stone's admirable papers on the transit of *Venus*, it occurred to me that experimental information as to the "black drop," which plays so important a part in this inquiry, may be obtained by means which I formerly used in some researches on irradiation, that were published in the 5th volume of the Society's *Memoirs*.

To obtain an artificial Sun I fixed a plate of brass, in which was a small circular aperture, in the focus of a good telescope. This was viewed, collimator-wise, with another telescope; and when illuminated by a lamp placed behind it, appeared as a luminous disk, 17′ diameter, sharply defined, and about as bright as the Sun was in the transit instrument. By interposing a piece of oiled paper the disk became much fainter, though the wires in the micrometer of the observing telescope were quite distinct. Making them internal tangents to the disk, and withdrawing the oiled paper, the limbs projected outside the wires from two to three seconds by the effect of irradiation.

Now an artificial planet may be obtained in two ways. 1. A small opaque disk may be carried by the frame of a micrometer in the observing telescope; it will appear as a black spot on the luminous disk, which can be moved at pleasure. Making the illumination of the aperture as faint as is consistent with distinct vision, the circumference can be brought into *real* contact and the corresponding micrometer reading noted. Then, using the full illumination, I expect that the "black drop" would appear, and that an *apparent* internal contact could also be produced. If the arm carrying the opaque disk be supposed likely to interfere with an exact appreciation of the phenomena, it might be supported by a central perpendicular wire an inch or two above the micrometer frame.

2. Or, which seems preferable, the opaque disk may be placed within the aperture in its plane, and moveable in that plane by a micrometer attached to the collimating telescope. This plan has the advantage that both Sun and planet are formed under precisely similar optical conditions, and that the apparent motion of the latter can be more conveniently imitated by attaching a driving-clock to the micrometer-screw.

The precautions which were required to give the aperture an edge which can bear high magnifying, are described in the paper referred to, and they would probably suffice to give an exact metal disk. I would, however, prefer one formed by drilling a conical hole through two pieces of glass cemented together, as described for the aperture; on separating them the edges of the sections which had been in contact would be as smooth as art can make them, and by filling one of them with black cement, a true



disk would be obtained, which could be placed exactly in the plane of the sun-aperture. The glass should be attached to the micrometer-frame, and thus no disturbance of the images by any influence of a support could occur.

If the illumination were made by magnesian or electric light, this apparatus would be also available for Photographic experiments on the Transit.

On the Increase of Probable Errors in a Transit of Venus as dependent upon the Smallness of Normal Velocity. By E. J. Stone, Esq.

In the *Monthly Notices* for April there appears a short paper of mine, "On the Comparative Clinging of the Limbs of *Venus* and the Sun in the Transit of 1874," and some consequences which appeared to follow therefrom.

In discussing the observations made in 1769, I convinced myself that the chief source of error to be feared was erroneous assumption of semidiameter. The errors arising from causes which chiefly affect ordinary transit-observations, such as errors in subdividing seconds of time, loss of time in mechanically registering impressions conveyed through the eye to the mind, &c. sink into insignificance compared with the three or four seconds which we had to allow for the probable error of a contact observation.

In observations of this kind the observers have to seize some well-marked phases of the gradual destruction of, and restoration of, the irradiation near the point of contact of the Sun's limb. The appearances are complicated by considerations of atmospheric disturbances, aperture of telescope employed, density of absorbing medium made use of to reduce the Sun's glare, eye of the observer, defining character of the telescope, and the power employed. But, under given conditions, the phenomenon — as, for instance, real contact or total disappearance of connecting ligament — will take place with a definite angular separation between the limbs.

If the attention is drawn to the first appearance of, or disappearance of, the connecting ligament, then that ligament must have a definite breadth, in order that, under the given circumstances of observation, it may be visible by contrast upon the Sun's disk. The question of seeing the ligament is not a question of sufficient time to enable us to pick it up, but of sufficient optical means to distinguish it. Such, at least, was and is my own opinion.

In the transit of *Mercury*, 1868, November, Mr. Carpenter, observing at Greenwich with a small telescope and low power, about 90, states that "the black ligament appeared to form *in-*



stantly, and to be of the breadth indicated, about $\frac{1}{3}$ the diameter of the planet." Now, although Mr. Carpenter observed the first formation, to him, of the black ligament, his time is about 13^s later than the time given by Mr. Lynn, observing with a power of about 170, and with a much better instrument. There was here plenty of time for Mr. Carpenter to see the ligament within the 13^s which elapsed between Mr. Lynn's observation and his own, but he did not see it, because, I maintain, his instrumental means were insufficient to appreciate the existence of the ligament by contrast, until it had become of a certain definite breadth. If this be the case, he would not have seen the ligament sensibly sooner had a period of 24^s elapsed whilst *Mercury* was moving over the distance from its position corresponding to Mr. Lynn's observations to that which it had at the time of his own observations.

The errors of such observations really arise when we assume that the phase of the phenomenon observed by one observer corresponds to the phase observed by another observer in such a way that each takes place at the same distance between the centres. The error thus committed is measured by the deviation of the true distance between the centres at which the observation was really made, and the mean distance for all the observations of apparently similar phenomena which are to be combined. The errors, therefore, measured in time, must be inversely proportional to the normal velocities. Another illustration, with a practical bearing, may be given, as follows:—Suppose that photography should be applied to the transit of *Venus*, by obtaining a great many photographs, by some quick process, whilst *Venus* is on and near the limbs, at points as near the points of maximum effect as possible. Then, with such a process, the errors in measuring the angular distances between the centres will be sensibly independent of the relative motions of *Venus* and the Sun; but if we should wish to apply these results to the determination of solar parallax by referring back all the photographs to some definite distance from the Sun's centre for comparison with photographs made at other stations, then, in referring these results back and determining the *time* at which *Venus* was at some definite distance from the Sun's centre, we should commit errors in *time* in proportion to the slowness of normal velocity.

These views led me to infer that the probable error of every single observation made in 1874 must be considered as injuriously affected by the slow normal velocity, and I gave in my April note the ratio of the mean probable errors arising from this cause in the transits of 1874 and 1882. I can understand differences of opinion existing with respect to the accuracy of these views; but I must confess my inability to find any full consideration of such views brought forward in the March number of the *Monthly Notices*.

On some Attempts to render the Luminous Prominences of the Sun visible without the use of the Spectroscope. By Warren De La Rue.

The progress of scientific discovery may be promoted by the record of one's failures, for it tends to prevent the same paths from being trodden by future explorers; on this account I venture to lay before the Society a description of some experiments on which I have been engaged since my return to Cranford in the spring of this year. Although these experiments have hitherto led to no satisfactory result, I am still pursuing them by varying the form of apparatus and the substances employed, in the hope of ultimately attaining the object in view. Notwithstanding the great success which has attended the study of the luminous prominences by Janssen, Lockyer, Huggins, Secchi, and more recently by Zöllner, it will be conceded that it is extremely desirable to find means of viewing these entities in their true position around, and on the Sun, without being compelled to distort the Sun's surface by spreading it out into an elongated sheet of light.

It must have undoubtedly suggested itself to many astronomers, that since the prominences emit light of but a few distinct and definite refrangibilities, it may be possible to absorb or to reflect out of the field of view the greater portion of all the other rays of the spectrum emanating from the Sun, by intercepting the light collected by an object-glass or mirror, by appropriate media. Indeed, the proposal of Huggins to combine the employment of the spectroscope with the use of absorbing media, naturally suggests the question,—why use the spectroscope at all?

The plan which I proposed to myself was twofold: to attack the F line on the one hand, and the C line on the other. In order to see the prominences by means of the F or green line, I adopted the following plan:—If the reader will suppose that in the case of a Newtonian, a right-angled prism is employed in lieu of the diagonal reflector, it will be evident that while the function of the hypothenuse is to reflect in totality the light it receives from the concave mirror, the two other sides of the prism are available for other objects, and may be covered by certain media through which the light must pass, and by which it may be altered in character by the reflection and dispersion into space of part of the rays, and the transmission to focus of others. The most promising covering medium for the right-angled surfaces of the prism appeared to me to be metallic gold, and fortunately the researches of Wernicke have furnished a means of obtaining a deposit of gold as brilliant and firmly adhering as the well-known silver deposits. The deposit of gold transmits a beautiful green, which includes the F line; and the Sun may be viewed without the slightest inconvenience after the light has passed through two films sufficiently thick. One advantage in gilding two surfaces of the prism consists in precluding the probability of any unaltered light from reaching the eye, for if minute holes occur in one film



it is not likely that others exist in the second film precisely opposite to them. The Sun's image when viewed through the gold films is beautifully defined, and perfectly cool, and very agreeable to the eye; indeed, if the hand be held in the principal focus of a 13-inch mirror, after the light has passed through the gold films, only a slight sensation of warmth is experienced; whereas a piece of wood, similarly placed, when the light is simply reflected by metallic mirrors or a prism not gilded, would soon be ignited: the gold film evidently, therefore, excludes most of the heat rays. Notwithstanding the hopes I entertained of rendering the prominences visible, I have not yet been able to succeed. There is, however, one circumstance which I ought to mention, because it may have contributed to the failure: it is this, it is extremely difficult to procure prisms in which the plane of the hypothenuse of the right-angled prism is truly parallel to the edge formed by the two sides at right angles. Very nearly true they are, but not perfectly so. In consequence of this imperfection, a great number of images of the Sun are seen (on account of a series of internal reflections), when the prism is held in the direction of the Sun; and although these may not overlap each other in the telescope, yet they cause an amount of diffused light which materially interferes with the possibility of seeing the prominences. The most obvious remedy is, of course, to try to procure a true prism; but the Astronomer Royal, to whom I imparted my difficulty, has suggested passing the light through pairs of prisms of small angle, superposed so as to form an optical monad, which would act as a parallel plate of glass, but in which the component prisms offer four sides which may be gilded. If one monad is not sufficient to screen away all the useless rays, then a second or a third might be used.

It may be mentioned that, in the case of the reflector, I have used a second smaller right-angled prism with two faces gilded, and placed between the eye-piece and the first prism before mentioned, and that the Sun's image was still very bright after passing through four films of gold. Also that I have employed a gilt prism with my 4-inch Dallmeyer.

It is not impossible that I may succeed in obtaining a photographic image of the prominences by using the gilt prism, even though I may not succeed in seeing them. Before, however, abandoning the attempt of rendering them visible by means of the gold films, I am having made sets of prisms, as proposed by Mr. Airy, and shall also endeavour to obtain true right-angled prisms; unfortunately, the Sun's low altitude will interfere for some months with the prosecution of the experiments.

Now, with regard to the C or red line, my plan of attack has been to employ red fluids, placed either between the eye-piece and mirror or object-glass, as the case may be, or between the eye-piece and eye. The two as yet experimented on without result are, a solution of carmine in ammonia, and a solution of aniline red in alcohol.



It is, of course, possible to ascertain beforehand whether the C ray is transmitted by the fluid, provided a suitable spectroscope is used; a trial showed that the red light transmitted by a solution of carmine in ammonia included the F ray, but this solution did not bring to view the luminous prominences. I am now preparing some pure carminic acid and some compounds of that acid for future experiments, and I am having a series of bottles made of larger capacity than those I have hitherto employed; for I have found that unless the light passes through one or two inches, or even more, of some of the coloured fluids, the eye is inconvenienced by the great brilliancy of the Sun's image. The vessels used have, of course, two sides of parallel glass.

On the Floor of Plato. By W. R. Birt.

During the last forty-eight years, occasional notices of the spots and marking on the floor of the walled plain *Plato* have appeared. In consequence of having given considerable attention to *Plato* and its surroundings in the years 1860 to 1863, I collected all the observations of the spots that I became acquainted with, numbering in the whole fifty-six, having reference to fourteen spots, two or three of which had been observed as craters, one being double. The greatest number observed ***simultaneously*** was seven, by Gruithuisen, in the year 1825. On the 23d of February of the present year, Mr. Pratt, of Brighton, observed with his 8-inch silvered-glass reflector eleven spots at the same time. They were not, however, eleven of the spots which had been observed previously, but included four unrecorded and a second double spot. Mr. Pratt has steadily continued his observations up to the present time, and determined the relative positions of twelve spots by alignment. Mr. Edward Crosley, of Halifax, has also kindly requested his assistant, Mr. Joseph Gledhill, to make continuous observations on them with his 9·3-inch achromatic by Cooke. The observations made by Mr. Gledhill and Mr. Pratt I have regularly received. The following are the names of the astronomers who have observed the floor of *Plato*. Gruithuisen, Mädler, Challis, Knott, the late Lord Rosse, the late Rev. W. R. Dawes, Baxendell, Dr. Dobie, Birt, Pratt, Crossley, Gledhill, and Elger, the number of observations being 297, in fifty series, and the number of spots observed twenty-five, including the companion of Dawes's double spot. In the annexed Table the number of each spot is given in accordance with the accompanying diagram, the name of the discoverer, the number of times each spot has been observed since the commencement of the present year, 1869, the comparative degree of visibility, that of the central spot, No. 1, being reckoned as unity, or 1·00, and remarks on the positions and general characters of the spots.

The contacts were observed with a power of about 90 on my 3¼-inch refractor, but the results are by no means satisfactory, owing to the irregular and ill-defined character of the shadow.

The colour of the shadow was very dark iron grey; the red tint usually seen in total eclipses was not noticed. Even with a power of about 30 and the illuminated disk excluded from the field of view, the details on the obscured portion of the surface were perceived only with the greatest difficulty. The eclipsed limb was, however, pretty distinct, but it appeared to be an arc of a smaller circle than the illuminated limb, the bending inwards of the limb at the cusps being very perceptible.

Windsor, New South Wales, August 11, 1869.

Comet 1869.

Discovered October 11, at Vienna, by M. Tempel.

Mr. Hind, in a letter dated Twickenham, Nov. 17, addressed to the President, writes, that Dr. Winnecke having sent him an observation on October 18, which fits in very well with two in the *Astronomische Nachrichten*, he has calculated the following approximation (apparently a very fair one) to the elements of the orbit:—

Perihelion Passage	1869, Oct. 8·4421 G.M.T.	
Longitude of Perihelion	124° 41′·1	Apparent
,, Ascending Node ..	311 24·4	Equinox.
Inclination	68 48·8	
Log. perihelion distance	0·0895	

Motion retrograde.

He remarks that these elements do not resemble those of any comet previously computed. They appeared in "*Nature*," No. II.

Occultations of Stars, and Eclipse of Satellite of Jupiter, observed at Leyton. By C. G. Talmage.

1869, July 21.—Occultation, disappearance of 33 *Sagittarii*; power 70 on 10-inch refractor. G.M.T. $12^h\ 3^m\ 23^s{\cdot}92$.

Good, time exact, cloudy at reappearance.

1869, August 2.—Occultation, reappearance of α *Tauri*; power 70 on 10-inch. G.M.T. $13^h\ 13^m\ 48^s{\cdot}23$.

Came out instantaneously, no hanging on limb. Moon hid by a tree at disappearance.

1869, October 11.—Occultation, reappearance of 31 *Sagittarii* on Moon's bright limb; power 70 on 10-inch. G.M.T. $6^h\ 7^m\ 55^s{\cdot}93$.

It was too light for disappearance, as light clouds covered the Moon.

1869, October 11.—Eclipse, disappearance of *Jupiter's* II. Satellite. G.M.T. $8^h\ 50^m\ 37^s{\cdot}21$.

Excellent definition, time good.

Mr. Barclay's Observatory, Leyton, Essex,
Nov. 11, 1869.

Instrument for Sale.

A 30-inch Transit Instrument by Troughton and Simms, for mounting on stone; $2\frac{1}{2}$ in. object-glass. Declination circle, divided on silver, with two verniers, reading to $\frac{1}{2}$ min. Three eyepieces, 37, 66, and 100. Diagonal prism. Micrometer to eyepiece, with Level, complete in case. Price 35*l.*; cost 55*l.* Apply to L. P. Casella, 23 Hatton Garden, London, E. C.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *December* 10, 1869. No. 2.

ADMIRAL MANNERS, President, in the Chair.

Charles Joseph Corbett, Esq., Imber Court, Thames Ditton;
Charles Lambert, Esq., Queen Street Place, Thames Street;
and
John Wood, Esq., Wharf College, near Tadcaster,

were balloted for and duly elected Fellows of the Society.

The November Meteors, 1869, observed at Port Saïd.

By G. L. Tupman, Esq.

On the morning of the 8th November, while observing shooting stars, I suspected radiation from the constellation of *Leo*. The following night was too cloudy for any observations whatever to be made.

On the 9th, between $12^h\ 45^m$ and $15^h\ 45^m$, radiation from *Leo* was observed at the rate of 8 per hour, principally small meteors. At the same time the radiation from two radiants in *Taurus* was at the rate of 12 per hour.

On the 10th, between 13^h and 16^h, the radiation from *Leo* was at the rate of about 6 per hour, and from *Taurus* about 7 per hour.

On the 11th, from $12^h\ 50^m$ to $15^h\ 10^m$, there was no radiation whatever from *Leo*, or indeed from any other point, although during that time shooting stars were observed at the rate of 16 per hour.



I am, as yet, unable to determine the radiant point in *Leo*, on the 9th and 10th, so that it is impossible to say if the meteors mentioned belonged to the true November system or not. Probably not.

On the 12th, out of thirteen observed meteors, four certainly radiated from near γ *Leonis*. One at $14^h\ 50^m$ passed exactly over the stars μ and α *Ursæ Majoris*, leaving a dazzling streak midway between those stars. This number reduced to the zenith would give about 5 per hour for a single observer.

On the 13th, a very strong breeze was blowing from the northward, accompanied by heavy clouds, which occasionally nearly altogether obscured the heavens. Above the lower cloud stratum was a thick haze which prevented the smaller stars being seen.

From $12^h\ 30^m$ to $13^h\ 15^m$, a pretty large patch being clear overhead, two meteors only were seen, neither of which radiated from *Leo*. It then became overcast, and I felt convinced that the shower was either all over or had not yet commenced.

The watch was resumed at $14^h\ 30^m$, the sky being then partly clear in patches, and continued until a quarter past 5, long before which the shower had entirely ceased. At $2^h\ 30^m$ it was at its height, most of the meteors being remarkably brilliant, and many of them tinted green. The greater part left bright streaks, which often remained visible a considerable time. The duration of the meteors or their "time of flight" was considered to be less than half a second—too short a time to estimate even roughly.

The following are the observations. Being unassisted, I stopped at every sixteenth to make the necessary entries:—

13 November, 1869, Alexandria Mean Time.

From h m	To h m	No. of Meteors.	Elevation of the Radiant. °
10 40·0	13 15·0	0	15
14 30·0	14 40·0	16	35
14 52·0	15 2·5	16	40
15 8·0	15 19·7	16	43
15 24·0	15 33·6	16	46
15 38·5	15 52·5	16	50
15 59·0	16 7·4	16	54
16 12·0	16 24·0	16	57
16 26·0	16 38·0	6*	60
16 40·0	16 52·0	7	63
16 54·0	17 14·0	4	67

Seven other meteors were observed, but they did not radiate from *Leo*.

If the numbers in the above table be reduced to an uniform

* During this observation it was more cloudy than before, but during the two following ones it was much clearer.



interval of time and then multiplied by the cosecant of the altitude of the radiant, it will be seen that between $14^h\ 30^m$ and $16^h\ 24^m$ the numbers were nearly uniform, and slightly decreasing. The maximum, then, was either before or about $14^h 30^m$; but the *centre* of the dense part must have been passed about 15 hours, as there was no sign of the shower at $13^h\ 15^m$.

For the determination of the Radiant point, eleven orbits were marked off on the chart, which appeared so close to the radiant as to give its position much more accurately than a host of orbits further off. The following table is a list of these orbits. The right ascensions and declinations are measured from the equinox of 1830, and, consequently, the resulting radiant point requires correcting for 39 years precession. Those of the first order of merit are more valuable than the others:

No.	Appearance.		Disappearance.		Order of Merit.
	α	δ	α	δ	
	°	°	°	°	
1	151½	+30,	152	+42	2
2	154½	+21¼	158	+21¼	1
3	161	+20¼	172	+19¼	2
4	152½	+19¼	157	+16¾	2
5	150	+19½	149¼	+14	1
6	149¼	+19	147½	+14½	1
7	140	0	144	−12	2
8	130	+7	117	−3	2
9	112	+5½	84	−8½	1
10	143	+19½	136	+16	2
11	140	+19½	127	+17½	2

These give the Radiant $\alpha = 151°{\cdot}0$, $\delta = 21°{\cdot}5$, measured from the equinox of 1869. But it must be noticed that no single point will satisfy all the paths that were observed, which proves that their orbits cannot be identical in inclination and eccentricity.

Three of the meteors deserve special notice.

The first occurred at $15^h\ 32^m$ near θ *Hydræ*. After describing an arc of 10° or 12°, almost instantaneously it exploded with a blaze of light that illuminated the whole sky, and was mistaken for lightning. At the point of explosion, a small luminous cloud was formed, about a degree and a half in diameter, of the shape of a nearly closed horseshoe, the interior diameter being about one-third the exterior. Its whole light was about equal to that of a first magnitude star, or of the nebula in *Lyræ*, seen in a large telescope. During the two or three minutes that attention was paid to it, it did not sensibly diminish its lustre or alter its place, which was $\alpha = 141°$, $\delta = +4°$.

The second occurred at $15^h\ 40^m$ in *Ursa Major*. It lighted up everything around as a brilliant flash of green lightning, and left an exceedingly bright streak some 8° in length, the centre of

which was in $\alpha = 131°$, $\delta = +55°$. After about a minute, this streak assumed a beautiful wavy appearance ~~~~, and two or three minutes after one of the shape of an S, the axis of which was inclined to the original axis about 70°. Five minutes after its first appearance it had drifted over the star ι *Ursæ*, and was still pretty bright when it was obscured by clouds.

The third was very bright, of a greenish hue, and exploded like the first, but behind thin cloud, which did not prevent its illuminating all around. Unfortunately, its position could not afterwards be identified.

On the 14th, from $14^h\,30^m$ to $15^h\,20^m$, about a third of the sky being very clear, five small shooting stars only were seen, radiating from *Orion*.

The thickness of the dense part of the stream must have been about 52,000 miles, measured perpendicularly to the plane of the orbit. It may only have been one aggregation among many, and may not have been situated centrally in the stream.

From these observations the elements of the orbit are

$$\pi = 62^\circ\,36'$$
$$I = 15\ 38$$
$$\Omega = 231\ 44$$
$$\iota = \cdot 9062$$

Motion retrograde.

Assuming a periodic time of 33¼ years.

Port Saïd, Lower Egypt,
16 *November*, 1869.

The Assistant-Secretary, Mr. Williams, read some extracts from the introductory remarks to the MS. work presented to the Society by him at the last meeting, being a translation of the accounts of comets observed in China from B.C. 613 to A.D. 1640, with the original text, of which introductory remarks the following is a brief abstract:—

According to Chinese tradition the Emperor Shin Nung, the successor of Fuh He, the founder of the empire, was the first who instituted astronomical observations. His reign commenced B.C. 3218. One of his successors, Hwang Te, is reputed to have been the monarch who established the mode of reckoning their chronology by cycles of 60 years, in use to the present day. The first of these cycles commenced B.C. 2637. He is also said to have discovered the lunar cycle of 19 years, by which the intercalary moons were to be regulated, from which it should appear that this cycle was known to the Chinese about 2000 years before it was introduced into Greek astronomy by Meton. These, however, must be looked upon merely as Chinese traditions, having no other authority.

In the most ancient of the Chinese historical works, the Shoo King, the instructions of the Emperor Yaou, who ascended the throne B.C. 2356, to his astronomers He and Ho are mentioned. In these they are, first, commanded to record their observations of the heavenly bodies, to complete the calendar, to make an instrument to show the motions of the Sun, Moon, and stars, and with due respect to impart information respecting the seasons to the people. In the succeeding four paragraphs four astronomers are sent to places in the east, south, west, and north, to observe the equinoxes and solstices, and certain stars are named as indicating the seasons. The first of these stars answers to our *α Hydræ*, which is described as culminating at sunset on the day of the vernal equinox. From this it seems that the vernal equinoctial point was then in the *Pleiades*, and consequently by the precession of the equinoxes it would appear that this observation was made B.C. 2306, being the 56th year of the reign of Yaou, thus affording a strong presumptive proof of the veracity of Chinese history as recorded in the Shoo King. The remaining three paragraphs relate to the labours of the other three observers; and, in conclusion, Yaou calls the attention of the astronomers to the fact that the year consists of 366 days, and among other matters of less importance to astronomy directs them to fix the intercalary moons, by which the seasons were to be kept in their places. Many other works are mentioned, both ancient and modern, as containing astronomical notices, and among these the great historical work called the *She Ke* and the Encyclopædia of Ma Twan Lin, are more particularly noticed as being those from which the greater number of the observations of comets contained in this volume have been extracted. The number of these observations thus brought together amounts to 370, a few of these may be meteors, but the far greater number are undoubtedly comets. Those mentioned by E. Biot in his communication to the *Connaissance des Temps* for 1846, amount to 224, consequently, in the present catalogue there are about 146 more than are given in that work; and there is reason to believe that the list now under consideration is as complete as any that has hitherto appeared.

The observations taken singly may be considered as divided into two parts, the one chronological, and the other astronomical. For reducing the first to our reckoning, certain tables have been constructed consisting of a complete set of chronological tables to be used in finding the years, and of others needed in reducing the moons and days. The construction and use of these tables are fully explained, and illustrated by examples of their application, and by their aid any year, moon, or day mentioned in the Chinese records can readily be reduced to our time.

For the astronomical portion, a complete celestial atlas, traced from the cuts in a Chinese work on astronomy, has been made, and which, as well as the preceding tables, forms a part of the work. In this the twenty-eight stellar divisions and the three

great spaces in which the stars are distributed by the Chinese are figured and described. To this atlas there is a full explanation, so that any group of stars mentioned in Chinese astronomy may be readily found by means of the index to the names; and the accordance of these groups with our constellations, is also made clear by means of reduced copies of the figures in Flamsteed's Atlas, with the Chinese asterisms laid down on the corresponding stars. There are many other interesting particulars mentioned in these introductory remarks which time did not allow to be entered upon, particularly an enumeration of the subjects treated of in the sections and chapters of the astronomical division of the history of the Ming dynasty 1368–1644, which, among other matters of interest, contain not only cometary observations, but also a catalogue of stars, with their longitudes and latitudes, both on the equator and the ecliptic, and more than seventy pages of observations of occultations of stars by the Moon and the planets.

Mr. Williams concluded by expressing his conviction that in placing this volume in the library of the Society he had secured it a position in which it is the most likely to be of service in future investigations into the subject of Chinese astronomy.

On Auroral Appearances and their Connexion with the Phenomena of Terrestrial Magnetism. By Balfour Stewart, F.R.A.S.

Some years since I ventured to suggest that auroral displays might be secondary currents due to small but rapid changes, caused by some unknown influence in the magnetism of the Earth. In developing this idea, the Earth was compared to the core of a Ruhmkorff machine, and the moist upper strata of the Earth, as well as the upper strata of the atmosphere, to secondary conductors, in which currents will take place whenever the magnetism of the Earth changes from any cause. These views would appear to be confirmed by the very interesting records of earth-currents obtained by Mr. Airy at the Greenwich Observatory, in which it is found that during times of great magnetic disturbance there are strong earth-currents alternating from positive to negative, the curves lying nearly equally on both sides of the zero.

A further development of this idea has lately occurred to me, in consequence of a remark of my friend Mr. Lockyer, that the zodiacal light may possibly be a terrestrial phenomenon, and may therefore be somehow connected with the phenomena of terrestrial magnetism. For not only will secondary currents be caused in a stationary conductor in presence of a magnetic core of variable power, but also in a conductor moving across the lines of force of a constant magnet. The question arises, have we on the Earth such moving conductors? In answer to this, let us reflect what takes place at the equator. When once the anti-trades have reached the upper regions of the atmosphere,



they will become conductors from their tenuity; and as they pass rapidly over the lines of the Earth's magnetic force we may expect them to be the vehicles of an electric current, and possibly to be lit up as attenuated gases are when they conduct electricity. May not these form the Zodiacal light?

Such moving currents will of course re-act on the magnetism of the Earth. We may therefore suppose that somewhat sudden and violent changes are likely to take place in the Earth's magnetism at those seasons at which the Earth's great wind-currents change most rapidly. May not this account for the excess of disturbances at the equinoxes?

Besides the anti-trades there are also no doubt convection currents caused by the daily progress of the Sun taking place in the upper regions of the Earth's atmosphere. May not these also be the vehicle of currents as they cross the lines of the Earth's force, and account, to some extent at least, for the daily variations of terrestrial magnetism? and may not this be the reason of the likeness observed by Mr. Baxendell between the curves denoting the daily progress of the wind and those denoting the variation of the declination magnet? Such currents, in as far as they are electric conductors, taking place in the upper regions of the atmosphere would not be felt by the earth-current wires at Greenwich, and I think Mr. Airy has noticed that this is the case. But the tidal wave represents a motion of a conductor on the Earth's surface, with two periods in one lunar day. This motion cannot produce a very great secondary current, but may it not be sufficient to account for the lunar-diurnal magnetic variation, which is also very small?

Such a current taking place in a conductor electrically connected with the Earth's upper surface ought to be felt by the Greenwich wires, and, if I am not mistaken, Mr. Airy has detected a current of this nature.

May we not also imagine that there are two varieties of Aurora, one corresponding to stationary conductors under a very rapidly changing core, and the other to rapidly moving conductors under a constant core? And might not an Aurora of the latter kind indicate the approach of a change of weather?

These remarks are thrown out in order to invite comment and criticism, and they will have served their purpose if they direct attention to the part that may be played by moving conductors in the phenomena of terrestrial magnetism. It will be noticed that these remarks do not touch upon the mysterious and interesting connexion believed to exist between magnetic disturbances and the frequency of solar spots.

P.S.—Since writing the above, Sir W. Thomson has called my attention to a paper by him in the *Philosophical Magazine* for December 1851, in which it is suggested that moving conductors may play a part in the phenomena of terrestrial magnetism.

Note on Mr. De La Rue's paper "On some attempts to render the luminous prominences visible without the use of the Spectroscope." By William Huggins, Esq.

Mr. De La Rue puts the following question, "The proposal of Huggins to combine the employment of the spectroscope with the use of absorbing media naturally suggests the question,—why use the spectroscope at all?"

The suggestion contained in this question was one of the methods by which I originally attempted to render the red flames visible. Some three or four years since, almost at the same time that the method of reducing the scattered light of our atmosphere by the use of dispersion occurred to me, the method by absorption also presented itself to me as a very feasible plan. I made many attempts, not only with the spectroscope, but also with variously combined coloured media without the spectroscope. The latter plan, namely, by absorption, appeared to me to be peculiarly desirable, if it could be accomplished, as it would enable the observer to see the forms of the prominences, and also the whole disk of the Sun at once. It could be applied to any telescope, or used with an opera-glass, or even with the naked eye.

I referred to this method in the report of my Observatory, *Monthly Notices*, vol. xxviii. p. 88. Subsequently, when the Indian observations had confirmed my suspicion that the prominences would give bright lines, and also shown their position in the spectrum, I gave in a short Note an account of this possible method of viewing the solar prominences: (*Monthly Notices*, vol. xxix. p. 4.) The method is also described in my paper in the *Phil. Trans.* 1868, p. 551.

This method was also independently suggested by Lieut. John Herschel in September, 1868: (*Monthly Notices*, vol. xxix. p. 5.)

During the early part of the present year I tried a large number of coloured media. The difficulty is to combine two media which shall absorb light of all refrangibilities except precisely that of the line C or the line F. If even a small range of refrangibility besides that of the line selected, say C, be allowed to pass, the scattered light of the atmosphere overpowers and eclipses the prominences.

The most promising media of those which I tried, were a solution of carmine in ammonia, which cuts off very nearly all the light more refrangible than C, combined with a solution of chlorophyll, which gives a strong band of absorption, taking away the brighter part of the light less refrangible than C. Unfortunately, however, the chlorophyll band encroaches a little upon C, and so weakens the light of the prominences. The absorption of chlorophyll, as Prof. Stokes has shown, can be moved a little in the spectrum by acids and alkalies, and differs slightly in different plants; but I have not been able to degrade the band sufficiently to allow light of the refrangibility of C to pass wholly unimpeded.

The proposal stated by Mr. De La Rue of combining the employment of the spectroscope with the use of absorbing media, refers probably to a method I proposed for rendering the *forms* of the prominences visible in the spectroscope. In a Note to the Royal Society, *Proceedings*, vol. xvii. p. 302, I showed that by using a wide slit, the forms of the prominences could be seen directly; and I then proposed using a deep red glass before the eye-piece of the spectroscope for the purpose of relieving the eye from the glare, which, under some circumstances, is painful, when a wide slit is employed.

On Spectroscopic Observations of the Transit of Venus in 1874.

By R. A. Proctor, B.A.

At the last Meeting of this Society it was mentioned that the American observers had suggested that spectroscopy should be applied to the observation of the approaching transits of *Venus*. It had been noticed during the total eclipse of last August that the approach of the Moon to the Sun's disk was rendered sensible before the actual commencement of the eclipse by the obliteration of the bright lines of the chromosphere; and the American astronomers suggest that the same might be the case when *Venus* is approaching the Sun's disk in 1874.

I think Mr. Huggins gave a practical character to this suggestion by pointing out that, although there would be difficulties in the method, applied merely to determine the motion of *Venus* over the chromosphere, yet that, as giving a means of preparing the observer for the occurrence of external contact, the method would perhaps be valuable. It is clear that this is, in fact, the only sort of observation which would have any value, since the chromosphere is not an envelope of uniform depth, and *Venus*, as seen from different parts of the Earth's surface, would appear to cross different parts of the chromosphere.

There is a further objection which I pointed out at the last Meeting in the fact that *Venus* approaches the Sun's limb not at a right, but at an exceedingly acute, angle, and that, as we do not know certainly the point of contact, we cannot tell the angle at which *Venus* will cross the limb.

Further, the stations best suited for observing internal contacts are not all suited for the particular mode of observation proposed by the American astronomers. At Crozet Island, for example, where internal contact most retarded can be most favourably seen, the Sun will be close to the horizon when *Venus* is traversing the chromosphere.

Passing over this last objection, which applies equally to egress and to ingress, it will be clear that Mr. Huggins' excellent suggestion that the passage of *Venus* over the chromosphere can be made the means of determining with extreme exactitude, the moment of external contact, can be applied satisfactorily to ob-

servations of *Venus* in egress, since the observer can follow the planet across the Sun's limb with perfect certainty as to the point of last contact.

It has occurred to me that the method of observing the prominences by means of an open slit (high dispersive power being used) might be adopted with advantage for determining the moments not only of external, but of internal contacts.

It is clear that so far as external contacts are concerned, any method which rendered the prominences and the chromosphere visible would serve to exhibit the passage of *Venus* over these objects, and so to indicate with extreme exactitude her first contact with the Sun's limb. I would invite particular attention to the extreme slowness with which, in 1874, *Venus* crosses the limb of the Sun. In a paper which appeared in the Supplementary Number of the *Monthly Notices*, I called attention to certain effects of this peculiarity. According to ordinary modes of observation these effects would be disadvantageous, though they are compensated by other advantages. But, according to the suggested mode of observation, the slowness with which *Venus* approaches the Sun's limb would be a great advantage. If we assign to the chromosphere an apparent height of about 10″, *Venus* will occupy about 4½ minutes in crossing that envelope, and this would afford the observer ample time to search for her. At egress, of course, it would be quite easy to follow the motion of her disk across the chromosphere.

In this plan the direction of the open slit would be at right angles to the Sun's limb; and it will be very obvious that internal contacts could be watched in the same way, and, perhaps, with more exactitude than by the ordinary method.

But it has occurred to me that internal contacts might be observed with advantage in a different manner, and probably Mr. Huggins had some similar plan in his thoughts, when at the last Meeting he suggested the use of a tangential slit.

If the open slit were placed tangentially, so as to include the solar cusps *a* and *b*, it is very clear that there would be seen two solar spectra separated by a dark space across which could lie the image of the chromosphere, which, however, would not be visible, owing to the brightness of the solar spectra. As *Venus* gradually moved from the Sun's limb the two solar spectra, or at least their inner edges, would appear to approach, and at the moment of real internal contact the dark space between them would disappear.



I think the moment of internal contact might thus be determined much more exactly than by direct observation. Of course, the method in reality amounts to changing the points of the cusps into parallel

lines and determining the moment at which these lines become coincident. But it seems obvious that, so far at least as optical difficulties are concerned, an observation of this sort would be much more easily made than an observation of "the breaking or formation of the dark ligament."

This method is clearly applicable to egress also, and applies as well to Halley's as to Delisle's method. The best stations for applying it are those referred to in my paper in the *Monthly Notices* for last June.

Nov. 22, 1869.

On a Change in the Colour of the Equatorial Belt of Jupiter.

By John Browning.

For several years I have been in the habit of observing *Jupiter* with considerable regularity with reflectors of large aperture. On every occasion previous to this presentation, the equatorial cloud-belt has been without colour, and the brightest portion of the disk of the planet. During the month of October this cloud-belt has, however, constantly been of a strong, greenish yellow, and darker than the bright belts north and south of it. The colour is almost exactly that known to artists as yellow lake. The colours of various parts of the disk I take from my observatory note-book.

Oct. 9th, 9·30.—Equatorial belt strong yellow lake, the other bright belts colourless. Dark belts very warm grey, and faint coppery red. The poles of the planet ashy blue. The drawing I have the honour of exhibiting represents the planet as it appeared at this time.

Two or three persons quite unused to the telescope noticed the colour of the equatorial belt.

Oct. 11th.—Observed *Jupiter* with Mr. Proctor. Colours the same, but the greenish-yellow belt was covered with white spots. Mr. Proctor agreed with me as to the greenish yellow of the equatorial belt and the ashy blue of the poles, but could not see the red in the dark belts.

In all instances I used 12 inches aperture and a power of 148, *achromatic* eye-piece.

I have received a letter from Mr. Brindley, of Lewisham, from which I give an extract:—"With reference to the appearance of *Jupiter*, I last night spent some hours with the 8½-inch telescope turned on him. I was most agreeably surprised; the belts were so different to what I had seen them before. The dark ones of a dark lake colour, the bright one of a lovely tinted green." As this letter was written some days after my drawing was made, and the writer had not seen the drawing, I cannot be much mistaken in the colours I have ascribed to the various portions of the planet.

Two years since I saw a coloured drawing of *Jupiter* at Mr. De La Rue's Observatory at Cranford. In this drawing, if my memory serves me, the equatorial belt was colourless.

I have watched the planet through the greater part of a revolution; the greenish yellow of the great equatorial belt remained unchanged. I have drawn the attention of several observers to the colours of the belt, some of whom have been in the habit of observing it for many years. One of these gentlemen says that he is certain that the equatorial belt has not presented such an appearance as at present for a quarter of a century.*

Writing to Mr. Proctor some time since, I said that I thought the very small specific gravity of *Jupiter* might be due to the existence of a cloudy envelope of enormous extent. Mr. Proctor replied, that he thought it just possible that the whole light from the planet might not be reflected; but that some portion might be emitted by the body of the planet. The same idea had presented itself to me. Being by far the largest planet in the solar system, it will certainly have retained more heat than the rest.

Such an alteration in the colour of the planet as I have described, must, I think, indicate some considerable change, either on the surface of the planet or in its atmosphere. In the hope of throwing some light on such changes in future, I am making a careful map of the spectrum of the planet. When this is completed I shall have the honour of submitting it to the Society.

Selenographical Notes: Apenninus and adjacent Regions.

By C. H. Weston, Esq.

Few lunar objects are more striking than the range of *Apenninus* lying S.E. of *Mare Imbrium*. It not only presents forms unlike the commonly existing circular structures, but when viewed on the *terminator* of an increasing Moon (and definition good) exhibits broad shadings on its massive flanks and serrated outlines mapped out in light upon the low lands far beneath.†

During the last month the decreasing Moon was examined under favourable circumstances on the morning of the 27th when just east of the meridian. Her age about $20^{d}\ 13^{h}\ 30^{m}$. By her libration a little more of the NNE. quadrant was made visible than in mean position. The *terminator* was particularly interesting, as its extremities passed so very near both the lunar poles. On or near the *terminator* the following striking points were carefully identified:—Region near North Pole, *Anaxagoras* on east of terminator, *Barrow A* on west (shadow fine), *Epigenes* on east, *Aristoteles*, *Eudoxus*, *Calippus*, on east, *Menelaus* on east, *Agrippa* on east, *Godin* on east, *Delambre*, *Taylor* on east, south-west end of valley between *Taylor* and b.c. south-east of *Taylor*, *Kant*, *Descartes*, *Abulfeda*, *Almanon*, *Sacro-bosco*,

* Since writing the above I have received information which leads me to suspect periodicity in the change of colour on *Jupiter*.

† During a partial eclipse (when illumined objects could be contrasted with dark parts) details were finely seen by the larger Newtonian (14·25 in. 16 ft.)

Gemma Frisius, Maurolycus, Clairaut, Jacobi, Pentland, Simpelius, and region north-west of South Pole. On the south-east *Sharp* could be detected, and the high mountain range north-west of *Newton.** The extreme south peripheral regions of mean Moon were cut off by libration.†

On 26th *Scoresby* (south-west of North Pole) and two contiguous *ellipses* were seen, but (by the same libration) under greater than the mean visual angles, and therefore apparently more circular than in Beer and Mädler's map. *Gioja,* too, and the mountainous range still further north, which conceals the North Pole point.‡

It is best, first, to describe the cliff-like coast line of *Apenninus* and adjacent regions areographically. The lower general surface of *Mare Imbrium* (except when broken by the rays of *Copernicus* and low ranges) is apparently continued at about the same level to *Apenninus,* and extends from *Eratosthenes* to the vicinity of *Huygens,* where, I conceive, should have been introduced in the valuable lunar map an equally dark continuous shading, as far as and around *Huygens,* to meet the shading at the foot of the promontory in the rear, and so sweeping round and onward to the ranges still more retired. At the last point begins the elevation of the floor of *Mare Imbrium,* and extends north-east towards *Timocharis* and round to *Archimedes.* I do not think, however, that this elevation should have been continued unbroken or so little broken. There seems to be an interrupted valley of some width, running north-east and south-west from *Archimedes* to *Apenninus,* co-extensive with the base of the former, and somewhat, perhaps, wider near the latter. Under low telescopic powers (in this phase of the Moon) it assumes not only a tripartite structure, but (at the end abutting against *Apenninus*) a broken shaded triangular form. Under high powers we perceive in it incipient or low protrusions, circular and rugged, and the shaded triangular part loses its character and is found marked with several hill ranges and deep dark combes. The *south-west patch* has considerable ridges, consisting apparently of great arcs of curves, connected like festoons with the convexities towards the valley, and together occupying about its whole length. It is true, indeed, that at full Moon the valley shows the brilliancy characteristic of raised lunar surfaces, but the experienced eye can see that while above the level of the *Mare Imbrium* it is lower than the contiguous patches.§

* A peak in the vicinity is the highest part of the Moon ("Der Mond," B. & M.)

† Under favourable libration and latitude the exact locality of South Pole can be observed through an opening in *Malapert* (B. & M.)

‡ On this range rises a peak sufficiently high to be *always* enlightened by the Sun and so to enjoy perpetual day, while the plains on the north base experience as constantly the ever-recurring vicissitudes of day and twilight (B. & M.)

§ Positive and negative photographs were taken of the Moon during last



On the north-west of the valley the elevated patch appears dome-shaped, and as *Autolycus* occupies the central position, the idea is suggested that the Ringgebirg was the ultimate result of the upheaving forces. At the north-west point above *Hadley* there is (as in the lunar map) a slight depression.

The entire Apennine region (Das Apenninen Gebirg und Hochland of B. & M.) is a large triangular district of which the promontory north-west of *Hadley* is the apex and the north-east converging side the one arresting our attention. In this side may be noticed about seventeen marked breaks, with the principal elevations standing out in high relief. Gigantic spurs (Vorberge, B. & M.), and rather high ranges are also to be seen running south-west, leaving profound intervening valleys. Beginning north, *Hadley*, *Aratus*, *Bradley A*, *Bradley*, *Huygens*, *Huygens A*, *Wolf A*, can be traced towering up more or less strikingly,—*Huygens A*, the highest (about 21,000 feet, B. & M.)

The physical characters of this part of *Apenninus* are those of a mountainous region, abruptly terminating on one side, running for a great distance in a course very direct, with subordinate ranges branching off at right-angles to the coast-line. These features present a structure very similar to that displayed on good maps of the Himalayan chain, where the great spurs and the lesser elevated lines of hills, are at right-angles to its main axis, while the sudden fall of *Apenninus'* coast-line to the level of *Mare Imbrium*, is also analogous to the Asiatic break-down of the high land of Thibet to the low level of Hindostan.

It is as important to the selenologist as it is to the geologist to obtain some approximate knowledge of the chronological sequence of the different formations (or modifications) of the lunar crust. The general appearance of *Mare Imbrium*, running up direct (and to so great an extent unchanged in area level) to the very foot of *Apenninus*, would lead to the inference that their physical condition existed anterior to the more local and limited upheaval of the small *patches north and south* of the valley of *Archimedes*. The *north patch* indicates that expansive forces had first produced the domical form, and then ultimately found vent in the ejection and building up the crater of *Autolycus*. These are symptoms of continuous, or at least, of protracted igneous action. *Archimedes* is probably posterior in age to the *south patch*, because the great ridging and elevation of high hill ranges seem to be rather connected with his protrusion. Again while it is clear that the *Apennine* range ran up to and beyond *Eratosthenes*, it is also probable that it once extended to *Copernicus*. If so, it would then follow that the elevation of this most magnificent Ringgebirg with its radial system, would, *pro tanto*,

month, and at the full Moon of this. On cursory views both showed decided triangular shading, but the translucent glass positives (much magnified) viewed both on collodionized and non-collodionized surfaces, and under different angles of light (*over black velvet*) distinctly showed the details of the *Archimedean* valley and the *south-west patch* ridged with hill ranges.

have obliterated the continuity of this range (as appears to be the case), and consequently be newer in age. On tracing carefully the direction of one of the north rays of *Copernicus* we find it to bifurcate near the *south patch*, and one of the branches to touch and become lost in or under it. There are often great niceties in judging of the effects of these rays, and inferences must be drawn cautiously, but, on the whole, I think that this branch underlies without ridging the patch. The connection of the raised parts with the branching and curvilinear elevations being already hypothetically accounted for, it would then follow that the ray is older than the south patch. Lastly, as to the remaining question of the relative ages of the *Apennine* range and *Eratosthenes*, I should consider this Ringgebirg as last formed, because its circular shape gives evident proof that nothing has interfered with its form since its upheaval, while the *Apennine* range seems to have been clearly interrupted by *Eratosthenes*. Hence, I infer the relative ages of these localities and districts to stand thus in a descending series (beginning with the newest), *Autolycus*, *Archimedes* (and perhaps the hill ranges on the south patch), *north and south* patches, *Copernicus*, *Eratosthenes*, *Apennine* range, *Mare Imbrium*.

Ensleigh Observatory, Lansdowne, Bath,
27 Sept. 1869.

On his new Observatory at Churt, Surrey.
By R. C. Carrington, F.R.S.

I have bought the freehold of almost nineteen acres of land situated six miles to the south of Fareham, on a part of Frensham Common, in the village of Churt. It contains a conical hill, sixty feet high, which is entirely detached, and it was this that induced me to purchase it. It goes by the name of The Middle Devil's Jump, as in another spot near by there is the Devil's Punch-Bowl. Its situation by the Ordnance Survey is

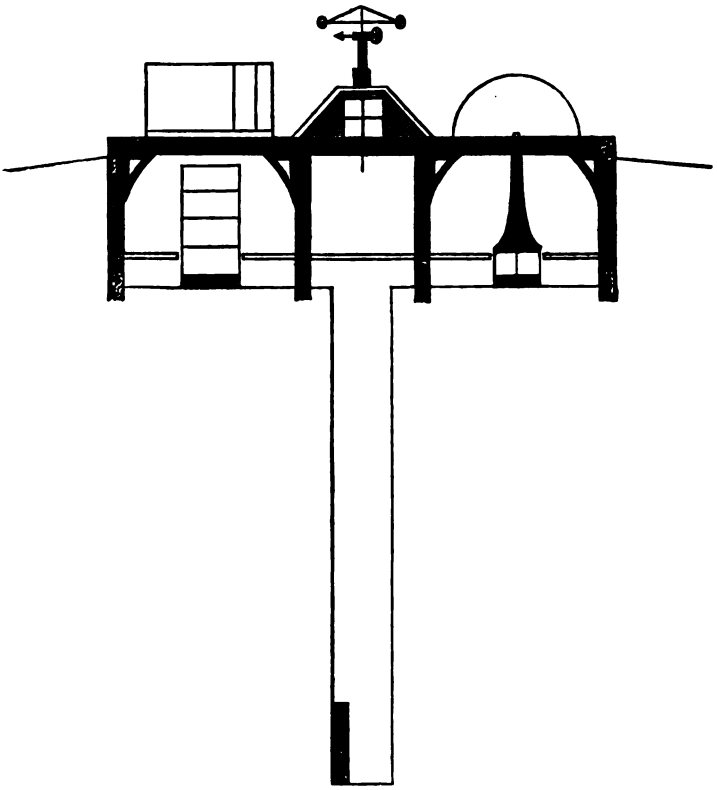
Lat. 51° 8′ 49″ N.
Long. $0^h\ 3^m\ 1^s{\cdot}7$ West of Greenwich.
Alt. about 340 feet above Liverpool.

Farnham is the nearest post-town and railway-station. It is necessary to state this, as I find letters and parcels continue to be addressed to me at Redhill, though I have left it seven years ago.

There are three things I wish to speak of, the Observatory in itself, the clock, and the principal instrument.

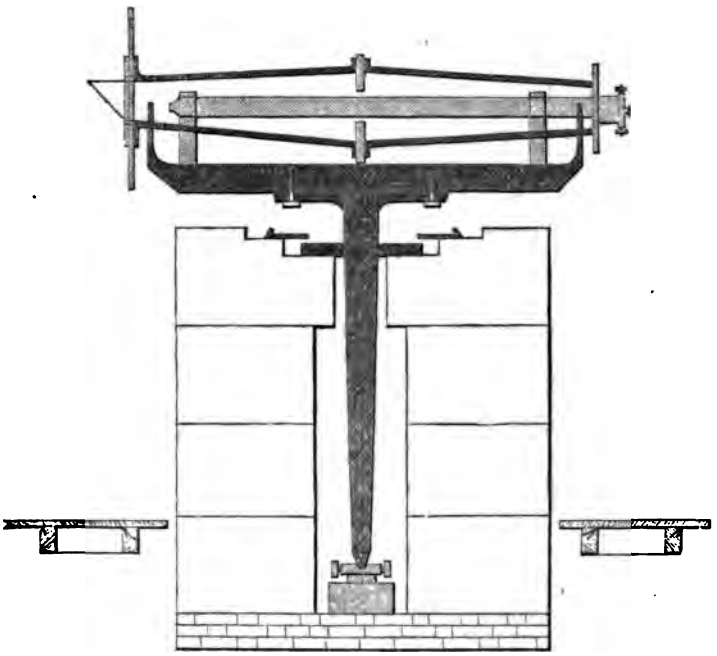
Being on a hill I did not want elevation, so I have sunk the

Observatory below ground, just peeping out over the soil. But I have further sunk a dry well, six feet in diameter, to the depth of forty feet from the centre of the Observatory, and with a horizontal shaft communicating with the south side of the hill, 166 feet in length, closed with three doorways. This is principally intended for the clock, for I am determined that one clock at least shall be properly mounted, at a position of invariable temperature and in an air-tight case. I propose to reduce the pressure to twenty-seven inches of mercury. My model clock is at present the one at Bidstone Observatory, made by the late

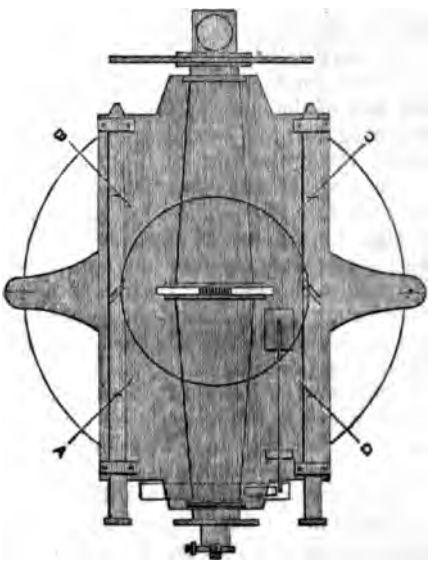


Observatory.

Mr. Richard Bond, with a gravity escapement, and its rates vary not more than 0ˢ·04 per diem in November and December. Still Mr. Frodsham, to whom I applied, assured me that, provided I had a good pendulum, he did not care what the escapement was, and I have reluctantly gone back to the old dead beat. The communication with the moveable dial above is by galvanic



Altazimuth. Dark shade steel or cast-iron, light brass or gun-metal.



Altazimuth viewed from above. Scale $\frac{1}{10}$.

wires, and all difficulty about hearing is overcome. I hope I shall shortly have the most perfect clock in England, perhaps in the world.

Of the principal instrument I have to announce that I have decided on having an altazimuth, but constructed on a new principle. I have fixed on Steinheil's principle of making the horizontal axis the effective optical axis, by placing the object-glass at one end, and the eye-piece at the other, with a prism outside the object-glass. The casting of the prism took over three months, but Messrs. Chance succeeded at last; six inches aperture is adopted, and six feet three inches focal length. It is some comfort to think that never need the telescope be raised, only turned round, and the observer always under cover. The principal vertical axis is steel, and the bed-plate of cast-iron. The telescope-tube is also cast-iron. The circles are both of thirty inches diameter; the altitude-circle of gun-metal solid throughout, that is without spokes, and cut out from a much larger piece. The microscopes are of five feet focal length, and I hope to try photography with them.

On certain important conclusions deducible from the Observations made on the Transit of Mercury at Greenwich, on November 5th, 1868. By Richard A. Proctor, B.A.

From a careful study of the investigations to which the observations made on the transit of *Venus* in 1769 have been subjected, and more especially of the masterly researches of Mr. Stone in the *Notices* for October 1868, I came long since to the conclusion that one of the chief points to be considered by astronomers in preparing for the coming transits is the effect which has been termed the "clinging of the limbs of the Sun and planet" near the time of true internal contact. In an article which appeared in the *Daily News* of November 4, 1868, I called special attention to the advantages which might result to science if observations were made on the internal contact of *Mercury* with reference to this effect. I wrote as follows: "Though transits of *Mercury* are not in themselves very important phenomena, it cannot be doubted that astronomers will avail themselves of the opportunity to practise, so to speak, for the approaching and far more important transits of *Venus* in 1874 and 1882. They will inquire whether the magnifying power of the telescope made use of has any bearing upon the duration of the deceptive appearance, or whether darkening glasses somewhat more powerful than those usually employed may not diminish the irradiation to which the phenomenon is due." A week later I wrote a letter to the same effect to the editor of the *Scientific Opinion*, and in a paper which appeared in that journal yet a week later (but was



in fact written at the same time as the letter), I called particular attention—first to the bearing of Mr. Stone's re-investigation of the transit-observations of 1769 on the subject of the coming transits; and, secondly, to the probable value of a "well-concerted plan of observations or even of a comparison, *inter se*, of observations already made on the peculiarity in question."

I mention these facts to explain the interest I take in the valuable series of observations made at Greenwich on the transit of *Mercury* in 1868, and particularly in the examination to which Mr. Stone subjected those observations. Certain highly important consequences seemed to me so obviously to flow from Mr. Stone's paper on the subject in the *Monthly Notices* for November, 1868, that in all cases where in treating the coming transits of *Venus* these consequences have been in question, I have in effect taken them for granted, not wishing to waste space in pointing out what Mr. Stone had already, as I conceived, demonstrated.

But as I judge from a passage in Mr. Stone's last communication to the Society that he himself reads his observations differently than I had been disposed to do, I feel that the importance of the whole subject will justify me in inviting attention to the conclusions which may, as I conceive, be directly deduced from Mr. Stone's "Remarks and Suggestions arising from the observations of the Transit of *Mercury* across the Disc of the Sun." (*Monthly Notices* for November, 1868.)

I refer now principally to the remark in his last paper, that the errors of contact-observations, "arise when we assume that the phase of the phenomenon observed by one observer corresponds to the phase observed by another observer in such a way that each takes place at the same distance between the centres." I had so read Mr. Stone's earlier paper as to believe its main object was to *prevent* observers from falling into this sort of error; and I still, after renewed and careful study, not only of that paper, but of the whole subject, can form no other opinion than that the practical value of Mr. Stone's researches is altogether greater than he seems to believe. Instead of looking upon his remarks as merely suggesting a difficulty, I find in them the clear indication of a method of escape from that difficulty.

To save space, I will take only the extreme cases of difference referred to in Mr. Stone's last paper.

Mr. Lynn, observing with the north equatoreal and power about 170, saw a delicate filament form itself between *Mercury* and the Sun's limb at $21^h\ 0^m\ 0^s{\cdot}7$ G.M.T. The filament was so fine that we may believe that Mr. Lynn really "caught a phase very near that of real internal contact."

Mr. H. Carpenter, using the east equatoreal and a power of 70, observed the sudden formation of a broad ligament between *Mercury* and the Sun at $21^h\ 0^m\ 11^s{\cdot}1$ G.M.T.; and on Mr. Stone's requesting him to point out which of certain figures (see *Monthly Notices* for November 1868) "appeared to him to represent most

nearly the appearance of the planet at the time of his observation, he without hesitation pointed out fig. (3)," but the phase, Mr. Stone says, was "somewhere between (2) and (3)."

Now in fig. 2 of the above-named paper, the ligament is rather less than one-half the diameter of *Mercury* in width, in fig. 3 rather more than one-half. We may suppose that in reality it was as nearly as possible one-half.

Therefore, supposing we wished to determine the time of real internal contact from Mr. Carpenter's observation, we ought, I apprehend, to proceed as follows.

We may assume, first of all, that irradiation does not tend to make the cusps seem appreciably nearer together than they actually are, because on no other hypothesis can we account for their squareness of outline. It follows, then, that the chord joining the cusps at the time of Mr. Carpenter's observation was equal in length to about one-half the diameter of *Mercury*. Hence it subtended at *Mercury's* centre an angle of about 60°, and therefore *Mercury* had really crossed the Sun's limb (normally) by a distance equal to *Venus'* radius × vers. 30°, approximately.

Now in the transit of Nov. 1868, *Mercury* traversed an arc of 41°, from external contact to external contact (Sun's semidiameter 16′ 10″·7, *Mercury's* 4″·9) in $3^h\ 38^m\ 18^s$. And it is easy to calculate from this *Mercury's* normal velocity, which I find was 0″·06412 per second. Hence the interval which had elapsed between real contact and the time when Mr. H. Carpenter observed the formation of the broad ligament, was

$$= \frac{4\cdot9 \,.\, \text{vers } 30^\circ}{0\cdot06412} \text{ seconds of time,}$$
$$= 10\cdot3 \text{ seconds.}$$

Hence the calculated epoch of real internal contact is—

$$21^h\ 0^m\ 11\cdot1^s \text{ G.M.T.} - 10^s\cdot3$$
$$= 21^h\ 0^m\ 0^s\cdot8 \text{ G.M.T.}$$

Mr. Lynn's observation gives, for the same phase—

$$21^h\ 0^m\ 0^s\cdot7$$

Hence, when thus treated (and there is nothing forced in the above method, nothing, in fact, which is not strictly accordant with Mr. Stone's interpretation of the nature of the clinging), Mr. Carpenter's observation gives a result *differing by only one-tenth of a second from Mr. Lynn's.*

I do not suppose for a moment that the closeness of agreement in this particular case is not in part accidental. But it appears to show very clearly that we can, by treating in the same way observations made on *Venus* in 1874 and 1882, obtain results *much* more accurate than by neglecting the considerations flowing from Mr. Stone's remarks on the transit of *Mercury*.

Of course, if those who come to manipulate the observations of

1874 and 1882 should insist on assuming that a phase caught by one observer is identical with a different phase caught by another, the old cause of errors will creep in. But in doing so they would be blinding themselves to the real value and significance of the observations made at Greenwich on the morning of November 5, 1868. This will not happen I should imagine. If, however, an Encke of the future should make such a mistake, doubtless there will not be wanting a Stone of the future to correct the results so obtained.

So long as the observers in 1874 and 1882 indicate as closely as they can the apparent breadth of the dark ligament, it must always be possible to determine the moment of real contact much more exactly in the manner above indicated than by assuming the phase observed to be actually a contact. It is for this reason that I have been always unwilling to accept the view that the probable error will be proportional to the slowness of the planet's normal velocity. I believe, on the contrary, that, during a transit of slow normal velocity, the time given to the observer to form several estimates of the breadth of the ligament, will counterbalance (not wholly, but to an important extent) the absolutely greater time-intervals which separate different phases of the internal contact. I have not before entered at length into the reasons which led me to this view, because they seemed so clearly deducible from Mr. Stone's valuable remarks on the transit of *Mercury*.

In the preceding paragraphs I have referred only to ordinary eye-estimates of the breadth of the dark ligament. It seems so obviously suggested that the eye-piece of each telescope made use of should be provided with an arrangement for estimating the breadth of the ligament — as compared with the diameter of *Venus*, that perhaps it may seem unnecessary that I should mention the point. Still, as I have found hitherto no reference to the necessity of this being attended to, I may be excused for remarking on it.

Micrometrical measurement is, perhaps, not desirable, though it would clearly be possible to apply it in an effective manner. It is not necessary, however, because absolute dimensions are not required. All we want is to know the ratio which the breadth of the ligament bears to the diameter of *Venus*. Hence if we adopt any of the numerous contrivances by which a series of cross-lines (or two series at right angles to each other would be even better) can be made to appear in the field of view with *Venus*, it will be the simplest possible matter for the observer to determine the relative breadth of the ligament — and this, even though at the moment of contact neither set of cross-lines should be absolutely normal to the Sun's limb at the place of contact.

If it should be thought advisable (as was suggested in November 1868 by the Astronomer Royal) that places of observation should be so selected that *Venus* will cross the Sun's limb either near its highest or near its lowest point, all that is requisite is,

that the place of observation should be taken as near as possible to the curved diametral arrow which lies across my two charts of the Earth in the *Monthly Notices* for June 1869 (plates 5 and 6). Most of the stations already dealt with are little affected by this condition; but, for observing retarded egress, all the Indian stations will be found (in this respect) far better than Alexandria. At Peshawur, for instance (a place already superior as respects coefficient of parallax and solar elevation), *Venus* will leave the Sun almost exactly at his uppermost point, whereas at Alexandria the point of last contact will be about 34° from the uppermost point of the Sun's limb.

A New Theory of the Milky Way. By R. A. Proctor, B.A.

Sir W. Herschel's respect for existing analogies—a quality which is perhaps of all others the safest guide for the scientific explorer,—led him to adopt as the means of interpreting his noble series of star-gaugings the hypothesis that there is a general uniformity in the distribution of the stars through space. He adopted this hypothesis not from a conviction of its being actually true, nor even from the belief that it is approximately so, but simply because existing analogies seemed to render it probable, and because it formed a convenient basis for calculation. The existing analogies were those presented by the solar system. In this system, Sir W. Herschel recognised a number of discrete bodies, not equal indeed, but comparable *inter se* in magnitude; not uniformly distributed, but still not aggregated towards one or another part of the solar scheme. And making such modifications as seemed requisite in comparing a system not regulated by a vast central orb with a scheme like our solar system, it seemed likely to him that a general equality of magnitude, and a general uniformity of distribution, might be found to prevail among the members of the sidereal system.

We now know that the ideas which astronomers had formed of the solar system in Sir W. Herschel's day were very far indeed from being correct. We see in the solar system a complexity of detail, and a variety of form, structure, aggregation, and motion, which were altogether unknown a century ago. And I cannot doubt that if the view we have of the solar system had been presented to Sir W. Herschel, he would have adopted as the basis of his star-gaugings an hypothesis differing altogether from that of which he actually availed himself. He would have argued, that as, in the solar system, there are bodies like the planets, far surpassing the other members of the scheme in magnitude and in importance; as it contains zones of minute bodies, such as the asteroids and the satellites composing the rings of *Saturn*; myriads of meteoric systems, and countless thousands of cometic systems, so doubtless in the sidereal system there are many forms of matter. If the analogy of the solar system is to be our guide, we

must look for suns equalling or surpassing our own in magnitude and splendour; for clusters and systems of minor suns, whose united mass may fall short of the mass of one of the primary stars; for aggregations of matter in portions relatively so minute as not even to be comparable with the small stars found in true star-clusters; and finally, for systems composed of materials, or at least of forms of matter, differing as widely from the substance of the suns, as the matter composing a comet does from the substance of the Earth or of *Jupiter*.

But, even independently of analogies such as these, his own series of observations led Sir W. Herschel to feel more and more doubt, as he proceeded, respecting the hypothesis which he had made the basis of his calculations. It is only necessary to compare the later papers he sent in to the Royal Society with the earlier ones, to find that views altogether inconsistent with his initial hypothesis were opening out before him. It was in those later papers that he spoke of star-groups in the Milky Way clustering towards opposite regions of the heavens; of stars arranging themselves into separate systems; and of the signs which the heavens present of the action of processes of aggregation, causing "the gradual dissolution of the Milky Way."

Sir John Herschel, also, in carrying out the system of star-gauging among the Southern stars, was led to notice many features which the hypothesis of a tolerably uniform distribution of stars could not satisfactorily explain.

Judged according to Sir W. Herschel's fundamental hypothesis, the sidereal system came to be regarded as forming a figure resembling that of a *cloven disc*, and the Milky Way was explained as being due simply to the greater extension of the system in the direction of the medial plane of this disc. Sir John Herschel, however, from his observations of the Southern heavens, was led to suspect that this theory was not strictly correct. He speaks in one place of certain evidence, according to which the Milky Way would come to be regarded as a flat ring seen edge-wise. And in many places he speaks of the difficulty of understanding certain features according to the views usually accepted.

It seems to me that the evidence collected respecting the Milky Way is sufficient to lead us to quite another view of its structure than that to which Sir W. Herschel was led by an hypothesis founded on the incomplete theories which astronomers in his day had formed respecting the solar system.

Let us regard the matter altogether independently of preconceived opinions, and judge simply as the evidence may seem to teach us.

In the woodcut, the outer figure represents the Milky Way according to the drawings and description of Sir John Herschel. The mode in which it is delineated needs no explanation.

Now in regarding this picture of the Milky Way, we are forced, I think, to the conclusion that neither the cloven-disc theory, nor the flat-ring theory, accounts satisfactorily even

for the principal features of the Milky Way. For example, the great gap which crosses it in *Argo*, nearly in the widest part of the single branch, seems utterly inexplicable on either theory. There is no way of accounting for that gap if we are really supposed to view the Milky Way from a point within its figure, and that figure resembles—however roughly—either a cloven disc or a flat ring.

But let us pass to other features. Travelling towards the right from the gap, we come to the strange semicircular cavity



with a well-defined outline, which Sir John Herschel has described in such striking terms. A cavity of that figure is a remarkable phenomenon, and is surely inexplicable either on the flat-ring or cloven-disc theory. But the mere distinctness of the outline is one of the strongest possible proofs that the stars which form that portion of the Milky Way constitute a distinct clustering aggregation from which we are separated by an enormous and comparatively star-less interval.

We come next to the Great Coal-sack near *Crux*, almost opposite to which is a well-marked opening in *Cygnus*. There are



also other strange openings through different parts of the Milky Way.

Now I cannot but think that an argument similar to that which Sir John Herschel has applied with so much force to the Magellanic clouds applies to the openings in *Crux* and *Cygnus*. He argues that because the Magellanic clouds approach roughly to the circular figure, therefore, in all probability, their real figure is that of a sphere: the chance is small that one of them is a cylindrical system seen endwise, but the chance that both are is altogether evanescent. Now applying the same principle to the Coal-sacks, we are led with equal certainty to the conclusion that these apertures are not cylindrical or tunnel-shaped openings seen endwise, but *if they are really openings at all* they are openings through a system which is not very much deeper—measured in the direction of the line of sight—than the greatest width of the aperture itself.

Judged in this way, the parts of the Milky Way which lie round a "Coal-sack" would have a roughly circular section, and not that enormous extension in the direction of the line of light, which has been assigned to them.

I cannot see that this argument is at all less sound or less effective than that which has been applied by Sir John Herschel to the Magellanic clouds.

There is another feature referred to, and I believe discovered, by Sir John Herschel, which is also full of meaning. I refer to the existence of narrow and sometimes convoluted streams of stars, branching out from the Milky Way itself. Sir John Herschel says of these that we ought to look on them as in all probability planes or scrolls of stars seen tangentially, and not as branch-shaped extensions bristling up from the general level of the Milky Way. And undoubtedly if the Milky Way really have a great extension in the direction of the line of sight, it is just that we should so regard these outlying streams. But if we judge of them without any reference at all to pre-existing theories, we are guided by strong arguments from probability to form a very different view. The chance that a plane system of stars, and still more a scroll of stars, should be turned so directly towards the Sun as to present to us the appearance of a straight or convoluted line or narrow stream of stars, is small indeed. The probability that several should be so situated may be regarded as evanescent.

Accepting these streams as having a roughly circular section, we are led to the conclusion that the Milky Way from which they extend has a similar section. In fact, as Sir John Herschel held these streams to be really planes or scrolls because (I assume) he assigned to the Milky Way a great lateral extension, so by inverting this argument I am led to believe that the Milky Way has not a great lateral extension (compared I mean with its thickness), because the streams extending from it have in all probability a section of roughly circular figure.



Other arguments there are that space will not permit me to dwell upon here, which point in the same direction.

Now, of course, if the Milky Way forms in reality a stream of stars *amidst the sidereal system*, the appearance which it presents upon the heavens might be expected to afford some information as to the shape of that stream, or at least of that portion which is cognisable by us. It must be admitted, however, that the problem of interpreting this wonderful stream is one of enormous difficulty. Perhaps it is one which men will never be able to accomplish in a perfectly satisfactory manner. It is only necessary to contemplate that marvellous maze of star-streams around *Scorpio* and its neighbourhood, and to read the account which Sir John Herschel gives (in his *Cape Observations*) of the telescopic aspect of this region, to feel how far we are at present from being able rightly to interpret the mysteries of the Galactic circle.

The bolder and more striking features of that circle may, however, be studied with a better hope of their being successfully interpreted. A theory which will explain the gap in *Argo*, the wide break of one stream in *Ophiuchus*, the varying brightness of the principal stream in different parts of its length, and other features of this kind, may reasonably be sought for.

I have endeavoured in the inner circle of the figure to indicate a spiral which seems to me to account for the most striking features of the Milky Way.

Following that spiral round from the part where the two loops approach each other, we have the following relations :

First of all, the gap is explained by the fact that the two loops do not meet. Then, remembering that the spiral is supposed not to lie in one plane, but (as the contorted figure of the Milky Way obviously suggests) to have been swayed out of that plane by varying attractive influences, we see that where the line of sight is directed tangentially to either loop, the Milky Way might be expected to have greater width than elsewhere. This explains the fan-shaped expansions on each side of the gap. Then on each side of these expansions we see the Milky Way double, which obviously corresponds to the relations exhibited by the two loops. Following the wider loop, we see that the double part of the Milky Way on this side extends nearly through a complete semicircular arc. The Coal-sack is explicable as due to the apparent intercrossing of the two contorted streams which really are at different distances from the eye.* The break in the further branch seems readily explicable as due to the great distance of a portion of this branch. But here the theory derives a singular support from the actual relative brilliancy of different parts of the Milky Way in this neighbourhood. Every astronomer knows how strangely the light of the Milky Way varies in and near

* In the large maps of the S. D. U. K. the Milky Way is depicted near *Crux* and *Argo*, as if the object of the draughtsman had been to support my theory. In Sir John Herschel's drawing, however, there are no such varieties of brilliancy.

Cygnus. The branch which extends from the Northern Coal-sack towards *Albireo* is at first far the brightest, and then fades off so much that in *Ophiuchus* it is wholly lost. The other branch, on the contrary, gradually increases in brightness, until in *Aquila*, and, further on, in *Sagittarius*, it forms the brightest part of the whole Milky Way. Now this part which is so very bright, corresponds to the part which my spiral brings so very near to the Sun.

Passing on to the termination of the second branch near *Cygnus*, it will be noticed how the spiral explains the strange extension of milky light from *Cepheus* towards the north pole.

Thence the stream is single, growing gradually fainter with increase of distance towards *Canis Minor* and *Monoceros*.

The spiral I have depicted seems so satisfactorily to account for several of the more striking features of the Milky Way, as to suggest the idea that it probably corresponds somewhat closely to the real figure of that star-stream. I am sensible, however, that many peculiarities remain unexplained by, though they are by no means opposed to, my theory. It must be remembered that any objections founded on a presumed equality of stars throughout the Milky Way, or of a general uniformity of distribution throughout the spiral stream, do not require to be met; because at the very beginning of this inquiry I have abandoned such hypotheses as inconsistent with existing analogies.

For example, there may be parts of this Milky Way so constituted, that if we were to remove further and further from them, we should see them gradually assuming the form of irresolvable nebulosity. But there may be other parts which would never assume that appearance, let their distance be what it might — the distribution and magnitude of the component stars being such that the stars would vanish through effect of distance, before the distances apparently separating them became evanescent.

I may add as a striking confirmation of a portion of these views, that among the lucid stars along the part of the Milky Way which lies nearest to the Sun, according to my view, are those which have been actually found to be nearest to us.

It must be understood that I regard the Milky Way as simply the condensed part of a spiral of small stars, which has been swayed into its present figure by the influence of large stars — the lucid stars seen in the Milky Way. The myriads of small stars not lying in or near the Milky Way, must yet belong to the same system, and in some instances seem to obey somewhat similar laws of aggregation. The nebulæ, so far as the evidence from probability extends, would appear to be groups formed from among those stars that have not fallen under the influence of the large stars which have brought the Milky Way spiral to its present figure. In the Magellanic clouds, we see the action of processes which have tended to form spherical clusters of enormous dimensions, in which both forms of aggregation are met with.

Why, in different parts of the sidereal system different pro-

cesses of aggregation should have taken place, we cannot yet distinctly see. But some of the striking discoveries which have recently been made by astronomers afford promise that light will soon be thrown on these perplexing questions.

P.S.—If my views respecting the Milky Way are correct, it obviously follows that there are parts of the Milky Way where traces of annual parallactic displacement might be looked for amongst *telescopic stars*. One instance of such motion would force us to modify all the views at present accepted respecting the sidereal system.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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WARREN DE LA RUE, Vice-President, in the Chair.

J. M. Eustace, Esq., Wimbledon;
J. E. H. Peyton, Esq., 63 Chester Square;
W. Garnett, Esq., Bashall Lodge, Clitheroe;
Capt. J. Williams, Aberdeen; and
Lieut. H. F. Yeatman, R.N., Sherborne, Dorset,

were balloted for and duly elected Fellows of the Society.

On the Eye-piece for Correction of Atmospheric Dispersion.

By G. B. Airy, Astronomer Royal.

In a late Number of the *Monthly Notices*, I showed that the effect of atmospheric dispersion on the images of celestial objects viewed at small elevations (to which I may add, the chromatic separation produced by slight error in the centering of the lenses of an object-glass), might be corrected by the insertion of a prism of proper angle in the eye-piece. It was obviously an inconvenience, though a very trifling one, that a battery of different prisms must be prepared for different elevations of the object viewed; and it would be better, if possible, to avoid the insertion of an additional piece of glass.

Mr. William Simms pointed out to me that a prism of adjustible angle would be produced by causing the convexity of a plano-convex lens to roll in the concavity of a plano-concave lens,

the radii of curvature of the convex and concave surfaces being equal. This construction would relieve the first of the inconveniences which I have mentioned, but would aggravate the second.

After various suggestions, Mr. W. Simms and myself arrived, independently and simultaneously, at a construction which, for simplicity and optical perfection, is not likely to be improved. It consists merely in making the eye-glass (supposed to be plano-convex) broader than is strictly necessary for the telescopic vision, causing it to press by its convex surface into a concave cup at the eye-end of the eye-piece, and allowing it to roll in that concavity; thus presenting different parts of its convex surface, though always in the same form and position, to the rays of light which come from the field-glass, but presenting to the eye a plane

Fig 1.

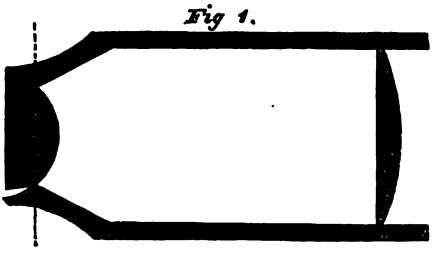
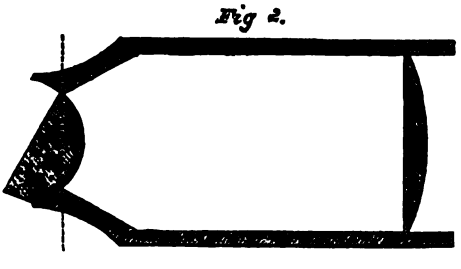


Fig 2.



surface, which, in one position of the lens, is normal to the telescope-axis, and, in another position of the lens, is inclined to it.

The above diagrams will explain the optical action of such an eye-piece.

Fig. 1 shows the position of the lens in the state for ordinary use.

Fig. 2 shows its position when atmospheric dispersion, &c., are to be corrected.

It will be seen that, in each case, the dotted line separates, on the telescope-side, a plano-convex lens of definite form; but that, in the first case, there is applied to it on the eye-side a piece of glass bounded by two parallel surfaces; and in the second

case, there is applied to it a prism, whose angle is gradually varied by gradually varying the position of the lens in its cup.

I have represented the large eye-lens as incomplete towards the lower edge. Optically, no injury is produced by making it complete; but I thought it possible that the projection of the lens edge might be inconvenient to the observer's eye; and also that the bluff termination of the lens might be convenient for the application of screw-motion.

The eye-piece in this state presents these advantages: it introduces no additional glass; it allows the use of a prism with angle gradually changing; and it does not disturb the corrections for spherical aberration.

It seems not too much to say that every telescope intended for delicate purposes might advantageously be furnished with such an eye-piece.

The eye-piece must have a swivel or rotatory motion round the axis of the telescope. A very simple screw arrangement, in a specimen eye-piece furnished to me by Mr. Simms, appears to place the eye-glass entirely under command.

1869, *Dec.* 20.

Seventh Catalogue of Double Stars, observed at Slough, in the years 1823–1828 *inclusive, with the* 20*-feet reflector;* 84 *of which have not been previously described.* By Sir J. F. W. Herschel, Bt. F.R.A.S. (Abstract.)

The observations of double stars herewith submitted to the Royal Astronomical Society were made in the course of my earlier sweeps at Slough, with the 20-feet reflector, and would have been included in the third of those Catalogues of double and triple stars observed with that instrument which the Society has already honoured with a place in its Transactions, but for a two-fold reason, viz., that that Catalogue was limited, 1st, to stars not before (to my knowledge) observed as double; and 2ndly, to the completion of a first exact thousand of such objects; thus causing the exclusion of 84, which are here entered as Nos. 5450–5533, and which in the subsequent redaction of Catalogues 4, 5, and 6, would appear to have escaped entry, owing to non-advertence to this circumstance. The remainder of the present list is made up, for the most part, of stars included in Struve's Dorpat Catalogue, and recognised as such. Although the angles of position of these latter stars are given only from careful estimation, they are not without some considerable historical value, inasmuch as in a great many instances they are antecedent in point of date to any recorded measurements, and in several are the only existing records of position; and though in some particular cases widely erroneous, they yet present for the most part such an agreement with subsequent micrometrical measures as in the earlier stages of this branch of astronomy would have ren-

dered their evidence available in deciding on the probability or improbability of a binary connexion between the individuals. Having included them, moreover, among the data which I have been for a long time occupied in collecting and arranging in a general Synoptic History of these objects; a reference to some recorded collective statement of them accessible without recourse to the original sweeps became unavoidable.

Summary of Sun-spot Observations made by the Kew Photo-Heliograph during the year 1869.

(*Communicated by Messrs. De La Rue, Stewart, and Loewy.*)

The following table exhibits our annual *resumé* of Sun-observations made at the Kew Observatory, drawn up according to the plan of Hofrath Schwabe, in Dessau:—

Months.	Days of Observation.	Days without Sun-Spots.	Number of New Groups.	Nos. given to the New Groups in the Kew Catalogue of Sun-Spots.
January	14	0	15	No. 902 to 916
February	15	0	17	917 933
March	11	0	14	934 947
April	20	0	15	948 962
May	16	0	18	963 980
June	18	0	27	981 1007
July	22	0	18	1008 1025
August	19	0	25	1026 1050
September	21	0	21	1051 1071
October	18	0	17	1072 1088
November	11	0	15	1089 1103
December	11	0	22	1104 1125
Total	196	0	224	No. 902 1125

Remarks.—The steady increase in the number of groups and the immense areas which many of them covered, points to the approaching maximum of the sun-spot period.

The year was also characterised by a remarkable tendency of the groups to appear in successive trains, within narrow and well-defined zones on both sides of the solar equator. Such a regular successive appearance of spots along parallels of latitude had previously been observed, but it usually only lasted during a short period, after the lapse of which the distribution in latitude became again irregular. Last year the irregularity of distribution was rather the exception. It is not improbable that a distinct law may be traced at some future time in this singular behaviour, and we recommend the subject to the attention of observers.

Kew Observatory, January 1, 1870.

On the Determination of the Orbit of a Planet from Three Observations. By Prof. Cayley. (Abstract.)

The author has proposed to himself to consider from a geometrical point of view the problem of the determination of the Orbit of a Planet from Three Observations. The Orbit is a conic, having the Sun for a focus, and each observation shows that the Planet is at the date thereof in a given line; we have thus a given point or focus, S, and three given lines, say the "rays." The orbit-plane, if known, would, by its intersections with the three rays, determine the three positions of the planet: that is, we should have the focus and three points on the orbit, or, what is the same thing, three radius-vectors from the focus, say a "trivector": geometrically, through three given points and with a given focus, there may be described four conics, but (as explained in the Memoir) only one of these can be the orbit; the orbit is thus determined, and that uniquely, by means of a given trivector. The problem therefore is to find the orbit-plane such that in the orbit determined by means of the trivector the times of passage between the three positions on the orbit may have the observed values, or (what is the same thing) that the orbital areas, each divided by the square root of the *latus rectum*, may have given values. Instead of the orbit-plane, the author considers the orbit-axis (that is, the line normal to the orbit-plane at the point S), or rather the intersection of this line with a sphere about the centre S, say the orbit-pole. To a given position of the orbit-pole there corresponds as above a determinate orbit, and the problem is to find the position of the orbit-pole such that in the orbit belonging thereto the times of passage may have given values, as already mentioned. The required position of the orbit-pole may be obtained as the intersection of two spherical curves, one of them the locus of the orbit-pole when the time of passage between the first and second points on the orbit has its proper given value, the other the locus where the time of passage between the second and third points has its proper given value; and in connexion herewith other isoparametric loci present themselves, for instance, the iseccentric lines, or loci of the orbit-pole such that along each of them the excentricity of the orbit has a given value. The object which the author has proposed to himself in the Memoir is the discussion of the configuration &c. of these loci. He considers, in the first instance, any three given rays whatever, but in the ulterior discussion of the spherical curves, which is carried out numerically, he has confined himself to the case of a particular symmetrical position of the three rays; viz., these are taken to be lines each at an inclination of 60° to a fixed plane through S; and such that their projections on this plane form an equilateral triangle having S for its centre, and that each ray cuts the plane in the mid-point of the corresponding side of the triangle. The results are exhibited graphically by means of the figures called spherograms, each the representation (on the stereographic projection) of the

half-surface of a sphere (not a hemisphere in the ordinary sense of the word, for there is great advantage in employing a different form of boundary)—viz., there is an *e*-spherogram, showing the iseccentric lines; and a T-spherogram showing the isochronic lines.

On the application of Photography as a means of determining the Solar Parallax from the Transit of Venus in 1874. By Richard A. Proctor, B.A.

It is impossible to read Mr. De La Rue's account of the results of careful measurement applied to photographs of the solar eclipses in 1860 and 1868 without recognising that we have in photography, as applied to the approaching Transit of *Venus*, one of the most powerful available means of determining the Sun's distance. Within the last few years, solar photography has made a progress which is very promising in regard to the future achievements of the science as an aid to exact astronomy. So that doubtless, in 1874, astronomers will apply photographic methods to the transit of that year, with even greater success than we should now be prepared to anticipate. It has therefore seemed to me that the photographic observation of the coming transit merits at least as full a preliminary inquiry as either Halley's or Delisle's method of direct observation.

The result of an inquiry directed to this end has led me to the conclusion that photographers of the approaching transit should adopt for their guidance considerations somewhat different than those which have hitherto been chiefly attended to.

It is undoubtedly true, as Mr. De La Rue has pointed out, that the photographer of the transit can readily take a large number of pictures, and by combining these, can ascertain with great accuracy the path of *Venus* across the solar disc. And by comparing the paths thus deduced for different stations a satisfactory estimate can be formed of the solar parallax. I do not wish to suggest any departure from this course of procedure.

On the other hand, it is undoubtedly true, as Major Tennant has remarked, that the greatest effect of parallax will be obtained for any two stations, when both stations, the Earth's centre, and the centre of *Venus*, are in one and the same plane. So far as those two stations are concerned, his remark is just, that it is the position of *Venus* at the instant when the stations are so situated, and not the nearest approach of *Venus* to the Sun's centre, which should be compared. And further, Mr. De La Rue's comment on this, to the effect that his method in reality includes Major Tennant's, is also correct. In fact, there can be no doubt that the position of *Venus* at the particular instant referred to by Major Tennant can be far more exactly ascertained by a reference to the complete path of *Venus* for each station than from any attempt to secure nearly simultaneous photographic records at stations far removed from each other.

But it appears to me that the method I am about to suggest, according to which the whole question will be reduced to the determination of a parallactic displacement of *Venus* on a line through the centre of the Sun's disc, is the one by which the fullest assistance will be obtained from photography; while a source of error, which has not hitherto been specially considered, will be practically eliminated.

It must be remembered that in the comparison of photographic records, whether for the determination of the path of *Venus* across the Sun's disc at a particular station, or for the comparison either of *Venus's* apparent position or of her path as seen from two different stations, the accuracy of the results will depend in part on the certainty with which two or more pictures may be brought into comparison by means of a fiducial line or set of lines. It seems certain that no method can be devised by which all chance of error from this source can be eliminated. The great point would, therefore, seem to be to render its effect as small as possible.

Now let us consider for a moment Major Tennant's proposition, as giving a convenient illustration of the effects of any error either in the position of the fiducial lines, or in bringing those belonging to two pictures into exact correspondence. Let Fig. 1 represent the result of a comparison between two photographs of the Sun. A B and C D are fiducial cross-lines common to both pictures, *a* is the centre of *Venus* for one picture, *b* is her centre for the other; and on the exact measurement of *a b* depends the determination of the Sun's parallax, so far at least as these two pictures are concerned. Now it is very obvious that if the lines A B, C D for one picture, have not been brought into perfect correspondence with those belonging to the other, the distance *a b* will be correspondingly affected. In fact, it would appear that if the usual methods for making the correspondence as exact as possible are followed, almost as large an error would be introduced through this cause alone as by errors in the measurement of *a b*, since the two processes—the measurement of *a b* and the adjustment of the sets of cross-lines—depend on the very same circumstance, the nicety, namely, with which the eye and the judgment can estimate minute quantities of about the same relative dimensions.

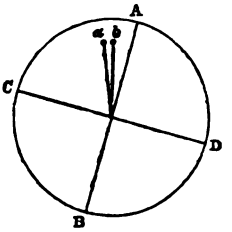


Fig. 1.

But now, if *a* and *b*, in place of having the position shown in Fig. 1, were situated as in Fig. 2, it is clear that the distance *a b* will not be appreciably affected by any small error in the adjustment of the fiducial lines.

The object, therefore, which it seems most desirable to secure is that *Venus*, as seen from two different stations at a particular instant, should have a relative parallactic displacement towards

ıe Sun's centre, or as nearly towards the Sun's centre as possible. This amounts to adding to Major Tennant's conditions this further one that the Sun's centre should be in the same plane with the two stations,—or rather to making this condition a substitute for that one which requires that the Earth's centre should be in the same plane with the two stations. For as a rule we must not expect to be able to secure that two convenient stations on the Earth, as well as the centres of the Earth, *Venus*, and the Sun, should be in the same plane.

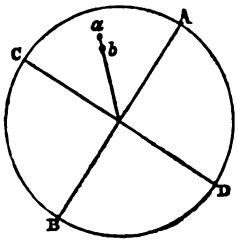


Fig. 2.

Mr. De La Rue's remark that by taking a series of pictures the position of *Venus* may be ascertained at any moment is in reality quite as applicable to my suggestion as to Major Tennant's. In fact, were it not, we might despair of securing the desired object, since we have no reason for believing that astronomers are so certain as to the exact progress of the transit, that the conditions could be secured by anticipatory instructions: whereas by applying Mr. De La Rue's method it will be possible after the transit is past, to determine with any desired degree of accuracy the position of *Venus* at the proper instant. And further, it is very obvious that no error in the placing of the fiducial lines for pictures taken at the same stations can much affect the accuracy of the result, since the comparison of successive pictures, taken at the same station, does not directly involve the element of the solar parallax, as when we have to compare two pictures or paths determined at different stations.

The object, then, of the present paper and the accompanying chart is to determine what stations are most suitable for applying photography to the transit of 1874, on the principles above enumerated. I think the drawing will be found, however, to be also an instructive illustration of the whole character of the transit.

In a paper in the *Monthly Notices* for March last, I showed how all the chief elements of the transit could be deduced by con-

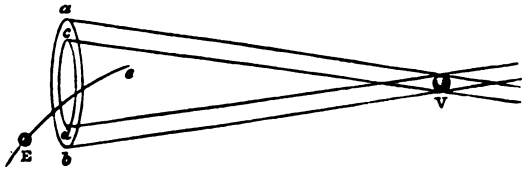


Fig. 3.

sidering the motion of *Venus* relatively to a pair of cones, each enveloping the Sun and the Earth, but one having its vertex outside the Earth, the other having its vertex between the Earth and the Sun. For my present purpose it will be convenient to con-

sider the motion of the Earth relatively to a pair of cones similarly enveloping the Sun and *Venus*.

Fig. 3 represents a portion of these cones around *Venus* at V. E *e* is the Earth's orbit relatively to these cones; *a b* is the circular section of the outer cone, *c d* that of the inner. *Venus* is supposed to be approaching the eye, and therefore the Earth also would, at the time of conjunction, be approaching; but as we are considering the Earth's motion relatively to the two cones, and as *Venus* moves more rapidly in her orbit than the Earth, we must suppose the Earth to traverse the sections *a b* and *c d* in direction E *e*. When the circumstances of the transit of 1874 are attended to, it results that the motion of the Earth relatively to the sections *a b* and *c d* is as shown in Fig. 4. The various circles represented along the parallels E *e* correspond to the various positions of the Earth represented in the illustrative plate.

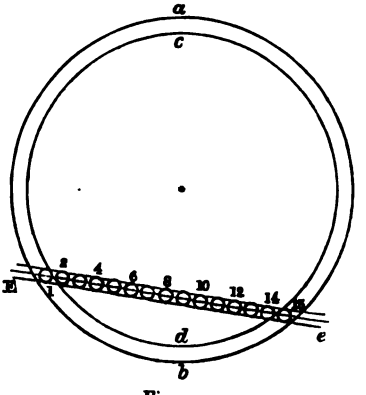


Fig. 4.

Comparing Figs. 3 and 4, it will be seen at once that as soon as the Earth reaches the outer circle *a b*, external contact begins. With the peculiarities of this phase, we need not concern ourselves. When the Earth reaches the circle *c d*, as at position 1, Fig. 4, internal contact begins, and when the Earth just touches the circle *c d* on the inside, the transit has begun for all places on the Earth's illuminated hemisphere. The positions 1 and 2 correspond to the cases of internal contact most accelerated and internal contact most retarded. They have been added to the illustrative plate for the sake of completeness, but in reality they do not belong to the special subject of this paper, since, as Mr. De La Rue has remarked, the photographer need not set himself to observe special phases of this sort. The same remark applies to the positions 14 and 15.

The remaining positions of the Earth in Fig. 4, corresponding to the 11 pictures 3–13 in the illustrative plate, are those occupied by the Earth at successive intervals of 15 minutes, the picture numbered 8 corresponding to the position occupied by the Earth at $16^h\ 6^m\ 31^s$ G.M.T., on Dec. 28th 1874, when *Venus* makes her nearest approach to the centre of the Sun's disc.

Now if we look at Figs. 3 and 4, and consider what they represent, we shall see that Fig. 4 may be looked upon as exhibiting an inverted picture of the Sun's disc and the transit of *Venus's* centre across it: we see, in fact, that the apparent position occupied at any instant by any point on the Earth's surface

in Fig. 4, corresponds exactly to the position occupied by *Venus* upon the Sun's disc, as supposed to be seen from that point of the Earth's surface at the instant in question. We have only to invert Fig. 4, and look at it from behind to see what sort of path *Venus* would seem to traverse upon the Sun's disc, either with reference to the Earth's centre, or to any point of the Earth's surface supposed to be properly depicted upon the small figures 1—15.

It follows, therefore, that if we want to determine two stations at which at any instant *Venus* would appear to have a relative parallactic displacement towards the Sun's centre, all that is required is that we select two stations which are on the same radial line from the common centre of the circular sections *a b* and *c d*.

The positions of those radial lines which cross the Earth's track through the section *c d* are exhibited in the plate. It will be understood, of course, that the three rows of figures belong in reality to a single row, the numbering of the successive pictures of the Earth indicating the way in which that row would be formed by the combination of the three rows shown in the plate.

I need not explain the construction of the plate, which depends on the simplest mathematical principles. I have taken a considerable amount of care to secure accuracy, not only in the projections of the Earth, but in the position of the radial cross-lines; and, though there may be minute inexactnesses, there will be found none, I think, which affect the purpose for which the plate was constructed. What that purpose is, will be best illustrated by simply examining the indications of the successive pictures.

Passing over pictures 1 and 2, we notice in Fig. 3, that Kerguelen's Land and Crozet Island, lying nearly on a line with certain of the Aleutian Islands, suggest that pictures taken at the former stations at the beginning of the transit could be advantageously compared with pictures simultaneously, or almost simultaneously, taken at a station on one of the easternmost of the Aleutians. In like manner pictures taken near Enderby Land could be advantageously compared with pictures taken at Woahoo. Projection 4 does not differ much from the preceding, but the cross-lines have assumed a less inclined position, and Kerguelen's Land could, at the epoch belonging to this picture, be better combined with a somewhat more westerly Aleutian island.

Projection 5 exhibits the advantage of a photographic station at or near Yokohama. Probably such a station, combined with one in Crozet Island or Kerguelen's Land, would give (by pictures taken near the hour belonging to Projection 5) absolutely the best results which photography can give.

The remaining projections suggest the following combinations of photographic records:—

Projection 6. Yokohama and Enderby Land, Kerguelen's Land



and a station in Manchooria; Crozet Island and Pekin; Cape of Good Hope and Nertchinsk.

Projection 7. Kerguelen's Land and Tsitsikar; Crozet Island and Nertchinsk; Cape Town and a station west of Lake Baikal.

Projection 8. Kerguelen's Land and Nertchinsk; Cape Town and Peshawur; Repulse Bay or neighbourhood and Yokohama; Perth (Australia) and Yokohama.

Projection 9. Repulse Bay and Yokohama; Enderby Land and Nertchinsk; Crozet Island and Calcutta; Cape Town and Bombay.

Projection 10. Repulse Bay and Nertchinsk; Possession Island (near South Victoria Land) and Yokohama; Kerguelen's Land and Calcutta; Crozet Island and Peshawur; Cape Town and Teheran.

Projection 11. Possession Island and Tsitsikar; Repulse Bay and neighbourhood of Lake Baikal; Enderby Land and Calcutta; Kerguelen's Land and Madras; Crozet Island and Peshawur; Cape Town and Aden.

Projection 12. Possession Island and Nertchinsk; Enderby Land and Madras; Kerguelen's Land and Peshawur; Crozet Island and Teheran.

Projection 13. Possession Island and neighbourhood of Lake Baikal; Repulse Bay and Calcutta; a New Zealand station and Yokohama; Hobart Town and a station near the mouth of the Amoor.

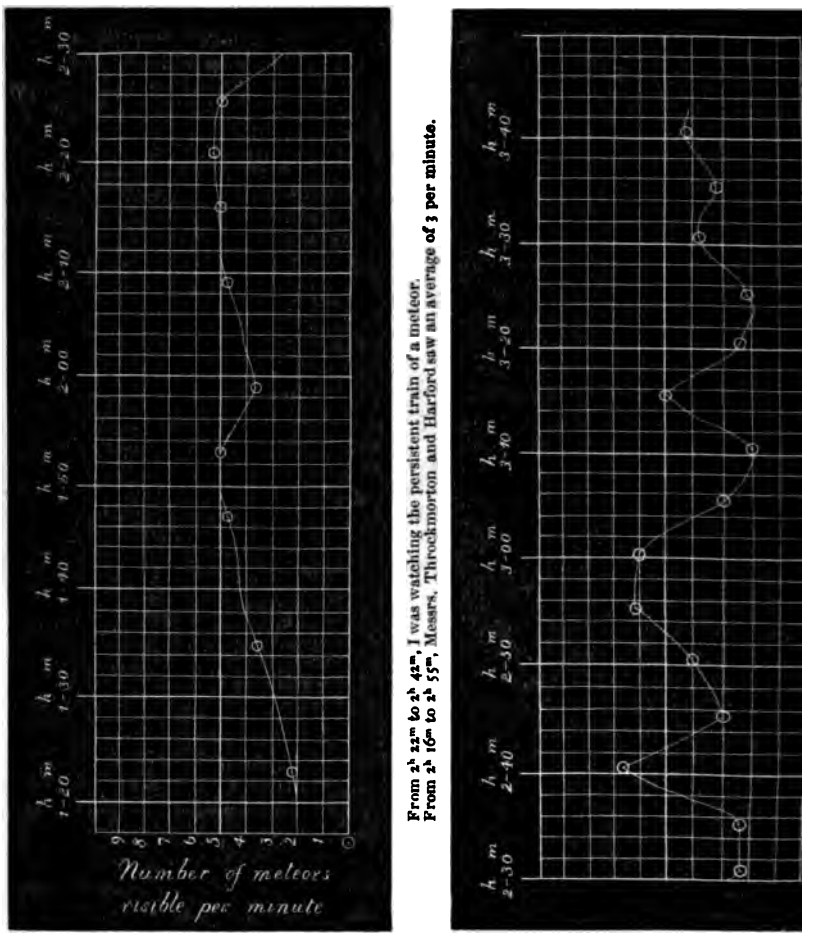
From this list we see that Kerguelen's Land and Crozet Island; Peshawur and other Indian stations; and stations in Siberia, are those which give the most favourable opportunities for the application of the photographic method.

Observations of Meteors, Nov. 13–14, 1869, *at Santa Barbara, California.* Observers, G. Davidson and Mrs. E. Davidson.

Watch was kept in camp for the meteoric shower; the night beautifully clear; Moon ten days old and 4° S. Decl. I was called at $1^h\ 10^m$, up to which time 22 meteors had been seen. The diagram exhibits the numbers noted by two observers up to $3^h\ 43^m$ A.M. After that time watch was kept up for any unusual display, but the numbers gradually diminished. The total number recorded was 556 in $2^h\ 25^m$. Some of the meteors were very brilliant and left persistent trains.

"At $1^h\ 25^m$ a meteor without apparent motion suddenly appeared, and burst in the same position. It was about 2° above and to the left of the bright star in the blade of the Sickle." (S. R. Throckmorton, Jun.)

At $2^h\ 33^m$ I observed a brilliant meteor start from a point above and a little to the left of the pointers; it left a persistent train and disappeared at a point about 9° or 10° above *Polaris*



and 6° to the right. The train was 5° in length; gradually it took a wavy form  ; then curved until it formed $\frac{2}{3}$ of an irregular circle  , and was 3° in diameter, and $\frac{1}{2}$ a degree in width. I examined it with a good binocular, and found it not of uniform density, but having open spaces in it. It remained visible 8½ minutes, and in that time apparently moved in a line towards the radiant point in *Leo*, over a space of 8°.

About half-a-dozen meteors were observed moving in directions towards the radiant. G.D.

Coast Survey Station, Santa Barbara, California.
Lat. 34° 24′ N., Long. 7h 59m.

Occultations of Stars by the Moon.

Observed at Maresfield, in Sussex, by Capt. W. Noble.

(Previously unreported.)

Monday, September 27, 1868.

Occultation of f Tauri.

The star disappeared (though not with extreme sharpness) at the Moon's bright limb at

$21^h\ 38^m\ 40^s{\cdot}5$ L.S.T. = $10^h\ 29^m\ 35^s{\cdot}5$ L.M.T.

Reappearance not seen.

Power 154 adjusted on the star.

Friday, February 19, 1869.

Occultation of 48 Tauri.

This star disappeared instantaneously at the Moon's dark limb at

$7^h\ 17^m\ 24{\cdot}3$ L.S.T. = $9^h\ 17^m\ 59^s{\cdot}5$ L.M.T.

and reappeared at the southern part of the bright limb about

$7^h\ 47^m\ 20{\cdot}15$ L.S.T. = $9^h\ 47^m\ 50^s{\cdot}8$ L.M.T.

Power 255 adjusted on the star.

Sunday, July 18, 1869.

Occultation of 49 Libræ.

The star disappeared about

$16^h\ 3^m\ 35^s$ L.S.T. = $8^h\ 16^m\ 53^s{\cdot}4$ L.M.T.

This was an unsatisfactory observation.

Power 255 adjusted on the star.

Wednesday, December 8, 1869.

Occultation of δ Capricorni.

The star disappeared instantaneously at the Moon's dark limb at

$22^h\ 45^m\ 34^s{\cdot}9$ L.S.T. = $5^h\ 35^m\ 23^s{\cdot}8$ L.M.T.

There was a great deal of haze.

Power 154 adjusted on the star.

I should state with reference to this observation, that, when I came to look at my slate by lamplight, after I had put down the instant of the star's disappearance, I found that I had, in some



unaccountable way, transposed the minutes and seconds. Had the *minutes* only been right, I should have taken the time of disappearance as 10 seconds later.

Tuesday, December 14, 1869.

Occultation of ξ^2 Ceti.

The star disappeared instantaneously at the Moon's dark limb at

$2^h\ 55^m\ 45^s{\cdot}3$ L.S.T. $= 9^h\ 21^m\ 26^s{\cdot}7$ L.M.T.

and reappeared at the bright limb, pretty sharply, at

$3^h\ 48^m\ 28^s{\cdot}6$ L.S.T. $= 10^h\ 14^m\ 1^s{\cdot}3$ L.M.T.

Power 255 adjusted on the star.

Having never yet made an independent determination of the geographical position of my Observatory, I have always employed data afforded by the Ordnance Survey, and assumed it to be situated in Latitude 51° 0′ 58″·3 North and Longitude $17^s{\cdot}5$ East. Our Fellow, Mr. F. C. Penrose, has now most kindly and obligingly computed my longitude from the above disappearance, and finds that, had such longitude depended upon this single observation, "it would have involved an error of only about 1300 yards." I hope, though, to refine upon this considerably.

Forest Lodge, Maresfield, Sussex,
14th January, 1870.

Observations of Jupiter's Satellites, and Occultation of Stars by the Moon. By John Joynson, Esq.

			G.M.T. h m s
15 Nov. 1869	1 Sat.	Ec. R.	7 52 32·0
	3	Tr. I. first	10 9 39
		last	10 38 52
		,, E. first	11 37 7
			11 56 23
22	1	Oc. D. first	7 14 15
		last	7 16 33
		Ec. R.	9 46 31·8
30	2	Ec. R.	5 11 45·0
	1	Tr. E. first	8 23 41
		last	8 28 20
		Sh. E.	8 56 55
			9 1 37
1 Dec.	1	Ec. R.	6 11 29·9

			G.M.T. h m s
3 Dec. 1869	3	Oc. R. first	7 54 10
		mid	7 56 15
		last	7 59 33
		Ec. D.	8 59 58·5
		R.	10 37 31·9
21	3	Tr. E. last cont.	4 58 32
23	2	Shad. full on	5 18 25
	1	Tr. I. first	6 4 7
		last	6 9 14
		E. first	8 9 24
		last	8 16 44
24	1	Ec. R.	6 28 5·5
31	1	Oc. D. first	5 3 47
		last	5 6 12
8 Jan.	2	Ec. R.	7 27 25·1
9	1	Ec. R.	4 49 38·2

Occultations of Stars by the Moon.

			G.M.T. h m s
10 Nov. 1869	30 Capricorni	Disappearance	7 0 30·6
17 ,,	μ Ceti	,,	9 48 19·7
		Reappearance	11 7 28·4

Waterloo, near Liverpool,
12th Jan. 1870.

On a Bright-Cross Micrometer for measuring the position of lines in faint Spectra. By John Browning, Esq.

Attempting recently to map out the spectrum of *Jupiter* I experienced great difficulty in determining the position of the lines. The spectrum was not sufficiently luminous to show the wire of the micrometer when it was opposite the red end of the spectrum, and it was in that portion that I wished to measure the position of the lines discovered by Mr. Huggins, which are probably due to the absorption of light in the planet's atmosphere.

As soon as I tried to light up the wire of the micrometer the spectrum became almost invisible, the lines being altogether lost.*

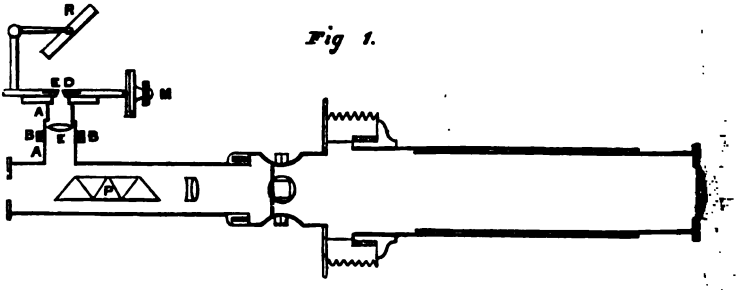
* Is the apparent brightness of *Jupiter* due entirely to the size of his disc? The spectrum of a star of the third magnitude is as brilliant even when widened by a cylindrical lens to the same breadth as *Jupiter's* spectrum.

Since writing the above, Mr. Proctor has kindly furnished me with an answer

To obviate this difficulty I contrived a bright-line micrometer, which is in action, and which I shall now have the honour to describe.

Fig. 1 represents the upper part of the star spectroscope. Attached to the side is a small tube, A A. At the outer part of this tube is a glass plate, blackened with two fine clear white

Fig 1.



lines across the centre, at an angle of 45°, like a letter X. These lines can, perhaps, be most neatly produced by photography.

The lens *c* which is focussed by turning the milled ring M, produces an image of the bright line in the field of view by reflection at the prism from the surface nearest the eye. On turning the micrometer screw, M, the slide which holds the glass-plate is made to travel in grooves, and the fine lines are made to traverse the whole length of the spectrum.

By means of the mirror, R, sunlight can be reflected into the micrometer. If desirable the light may be modified by placing a piece of silver paper spread with paraffin in front of the glass-plate.

Fig. 2 represents a spectrum, and shows the bright cross of

Fig. 2.



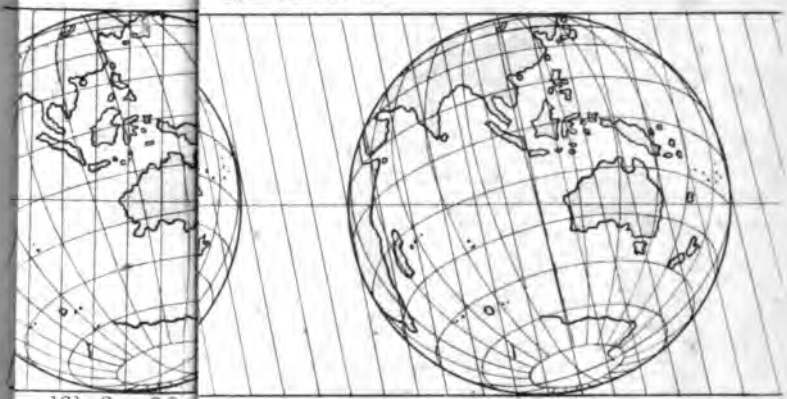
the micrometer in measuring a fine line in the most refrangible portion, where a wire or web would be imperceptible.

to this question, I had overlooked the obvious fact that while we have a star in the spectroscope, we obtain the whole light of the disc of the star, between the jaws of the slit, while, in the case of *Jupiter*, we have only the light of that portion of the disc of the planet to deal with which falls within the jaws of the slit. I would, therefore, propose to use the cylindrical lens, such as are used with the star spectroscope, in broadening the stellar spectra, but with the convexity of the lens turned in the other direction, that is to say, in the direction of the line of the slit instead of at right angles to it.

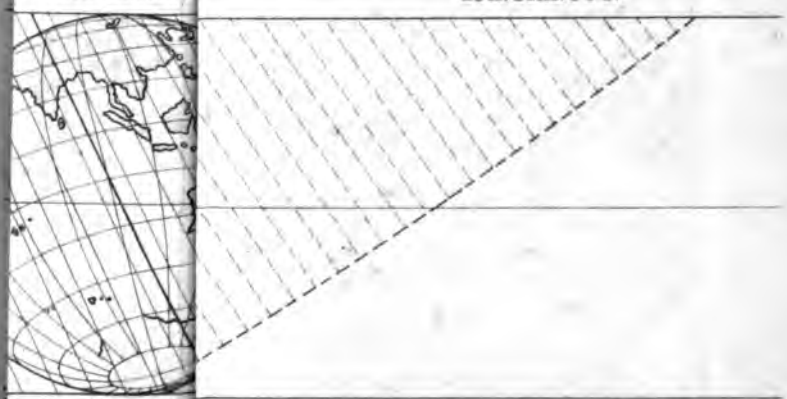
UGH V



14h. 29m. 5s. 15h. 6m. 30s.



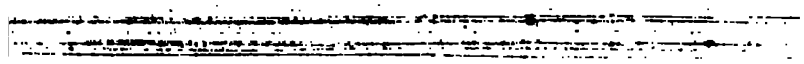
16h. 6m. 30s 16h. 51m. 30s.



17h. 43m. 58s

Malby & Sons, lith.

ılar section
nt of Venus



A Method of Constructing Charts by which in a few moments the Great Circle Course between any Two Points on the Globe may be accurately Obtained. By Richard A. Proctor, B.A.

It seems to me almost certain that the plan I am about to describe must have been thought of often before because it is so obvious. But as I have never seen any reference to it in works or papers where such reference was to have been looked for, I venture to bring it before the notice of the Society as having apparently a useful bearing on the problem of great circle-sailing.

It is well known that by Mr. Towson's tables the great circle course from one place to another may be calculated with considerable ease; but a construction by which such a course might be laid down on a chart is, as far as I know, still wanting. I remember having seen a paper (I believe by the Astronomer Royal) in which the difficult problem of laying down great circle courses on Mercator's charts was attacked. Later a short note appeared in which Sir John Lubbock showed how a construction founded on the principles of the gnomonic projection might be applied to the problem. I cannot at present recall where I read those papers, but I have an impression it was in the *Monthly Notices* or else in the *Memoirs* of this Society.

The fact that in the gnomonic projection the great circle course between two points is obtained by simply drawing a line through those points, is inviting. But as one cannot include in a chart even a complete hemisphere on the gnomonic projection, there is an obvious difficulty, which, as far as the purposes in question are concerned, renders the projection almost useless. And, in passing, I may note that so far as I am aware, no one has hitherto pointed out how the course between two points, one in the northern and one in the southern hemisphere, can be deduced (in great part) from gnomonic projections of the greater part of these hemispheres. The solution of the problem is exceedingly simple:—In the Northern map one must draw a straight line through the Northern station and *the antipodes* of the Southern; and the like with the Southern map. These two lines are the projection of the great circle through the points, so far as it falls within the maps. There can be no difficulty in selecting the segments which belong to the great circle course between the points.

The plan I have now to deal with has reference to the stereographic projection. In this projection every circle on the sphere appears as a circle. Every great circle is distinguished by the property that its points of intersection with the great circle around the centre of projection lie on a diameter of that circle. This in effect gives the geometrical construction for laying down a great circle through any two points on the projection. But

practically the way in this can be done on a chart is much simpler.

Every great circle through a place passes through the antipodes of that place. Now, suppose we have a stereographic projection of all the sphere except the south polar region; the north pole being the centre of projection, and we wish to find the great circle course between any two stations:—We find these two stations on the map, and we find the antipodes of *one* station; then a circle carried through these three points is the great circle required.

It would not be necessary to make any construction for determining the centre of the circle through the points. A few moments' trial with a rod-compass would give the circle quite as exactly as the usual construction. I may remark, however, that a useful addition might be made to mathematical instruments, in the form of a ruler by which a cross-line bisecting any given line at right angles might be drawn without any construction. Many forms will suggest themselves. One of the simplest and most convenient is founded on the property that the diagonals of a rhombus bisect each other at right angles. Very likely such instruments are made.

In such a projection as I have spoken of, there would be no occasion to include any regions south of the 50th or 55th parallel of south latitude. Nor need lands and seas be marked in unless it were convenient. If only the meridians (radial lines) and latitude lines (concentric circles) were marked in to every 5th degree (or to every degree, if need were) the deduced great circle course could be transferred in a few seconds to the Mercator's chart, supposed to be similarly divided.

Comet II., 1869.

(Tempel's, see p. 27.) The following elements calculated by M. Leveau from observations at Leipzig, 23 October, and Vienna, 13 and 31 October, are given *Astron. Nach.* No. 1783.

T = 1869, Oct. 9, 55102, Paris M.T.

	°	′	″
π =	139	20	43·6
Ω =	311	27	52·0
i =	111	32	54·0

$\log q = 0{\cdot}090056$

The mean observation is represented as follows,—

$O-C$; $\Delta\lambda = +0''{\cdot}5$, $\Delta\beta = +1''{\cdot}6$.



Two other observations at Vienna, October 12 and 27, are represented within some seconds of arc.

Comet III., 1869, *discovered by M. Tempel at Marseilles, 3 Nov.* 1869.

The following elements, calculated by Herr L. Schulhof from observations, Vienna, Nov. 29, Bonn, Dec. 4, and Cracow, Dec. 9, taking account of parallax and aberration, are given *Astron. Nach.* No. 1788.

T = 1869, Nov. 20, 37996, Berlin M.T.

$$\left.\begin{aligned} \varpi &= 40^\circ\ 37'\ 18''.4 \\ \Omega &= 292\ 56\ 45.4 \\ i &= 6\ 56\ 9.9 \end{aligned}\right\} \text{Mean equinox, } 1870.0.$$

$\log q = 0.042520.$

The mean observation is represented as follows,—

$$O-C;\ \Delta\lambda \cos\beta = +7''.1,\ \Delta\beta = +34''.0.$$

Instrument for Sale.

Transit-circle by Jones. The circle 18 inches diameter, divided on silver to 5′, reads with vernier to 3″. Two setting circles divided on silver. Object-glass 3¼ inches diameter, by Tully. On a portable iron stand. Apply to Mr. Huggins, Upper Tulse Hill.



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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *February* 11, 1870. No. 4.

Professor J. C. ADAMS, Vice-President, in the Chair.

W. A. Harris, Esq., Balliol College, Oxford,
was balloted for and duly elected a Fellow of the Society.

Report of the Council to the Fiftieth General Meeting of the Society.

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patroness and Honorary.	Total Fellows.	Associates.	Grand Total.
December 31, 1868	182	296	12	3	493	45	538
Since elected ...	6	29	...	...	...	...	...
Deceased	−4	−5	−1	..	...	...	...
Removals	+3	−3	...	...	...	...	...
Resigned	...	−6	...	...	...	...	...
Dec. 31, 1869 ...	187	311	11	3	512	45	557

Mr. Whitbread's Account as Treasurer of the I

RECEIPTS.

	£	s.	d.	£	s.
Balance of last year's account	390	14	3		
Error in the Petty Cash Account	0	0	4		
				390	14
By Dividend on £3000 Consols	43	17	6		
By ditto on £5000 New 3 per Cents ...	73	2	6		
By ditto on £3200 Consols	47	0	0		
By ditto on £5000 New 3 per Cents ...	73	8	9		
				237	1
On account of arrears of contributions ...	135	4	0		
155 annual contributions	325	10	0		
32 admission-fees	67	4	0		
28 first years' contributions	42	0	0		
				569	1
10 compositions	...		...	210	
Sale of publications:—					
At the Rooms of the Society	10	2	6		
By Messrs. Williams and Norgate, Publishers	23	16	8		
				33	1

£1442



stronomical Society, from January 1 to December 31, 1869.

EXPENDITURE.

	£	s.	d.	£	s.	d.
Salaries:—						
Editor of Publications	60	0	0			
Assistant Secretary	130	0	0			
,, ,, Gratuity	20	0	0			
Commission on Collecting	29	0	0			
				239	0	0
Taxes:—						
Land and Assessed	3	5	6			
Income	2	10	0			
Poor Rate	3	10	10			
Other Parish Rates	3	2	6			
				12	8	10
Bills:—						
Strangeways and Walden, printers ...	282	9	8			
Rumfitt, bookbinder	12	19	7			
Basire, engraver	54	10	0			
Pound, ,, ...	117	12	0			
Malby and Son, lithographers	98	2	1			
Cundall and Co., photographers	3	8	0			
Insurance	7	15	6			
				576	16	10
Miscellaneous items:—						
House expenses	23	1	9			
Postages	43	16	11			
Books and parcels	3	7	9			
Expenses of evening meetings	13	13	0			
Waiters attending meetings	3	17	0			
Coals and wood	12	0	0			
Gas	6	13	7			
Repairs	2	8	6			
Sundries	12	1	4			
				120	19	10
				949	5	6
Lee Fund	6	0	0			
Turnor Fund	1	6	0			
Mrs. Jackson Gwilt's annuity, 1 year ...	8	15	0			
				16	1	0
Investment:—						
Purchase of £200 Consols	186	0	0			
,, ,, ,, ...	187	15	0			
				373	15	0
Banker, Noting Bill				0	1	6
				1339	3	0
Balance at Banker's				102	17	6
				£1442	0	6

ROBT. J. MANN, M.D.
C. G. PRIDEAUX,
F. C. PENROSE,
} *Auditors.*



Assets and Present Property of the Society, January 1, 1870:—

	£	s.	d.	£	s.	d.
Balance at Banker's	...		...	102	17	6
1 Contribution of 8 years' standing ...	16	16	0			
8 ,, 6 ,, ...	100	16	0			
13 ,, 5 ,, ...	136	10	0			
4 ,, 4 ,, ...	33	12	0			
19 ,, 3 ,, ...	119	14	0			
20 ,, 2 ,, ...	84	0	0			
40 ,, 1 ,, ...	84	0	0			
Balance of an Account...	2	12	0			
2 admission-fees, and subscriptions ...	13	13	0			
				591	13	0
Due for Publications				2	14	0
£5000 New 3 Per Cents (including Mrs. Jackson's Gift, £300).						
£3400 Consols, including the Lee Fund (£100) and Turnor Fund (£500).						
Unsold Publications of the Society.						
Various astronomical instruments, books, prints, &c.						
Balance of Turnor Fund (included in Treasurer's Account)				139	12	0

Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	13	XII.	192	XXV.	199
I. Part 2	53	XIII.	202	XXVI.	204
II. Part 1	71	XIV.	394	XXVII.	459
II. Part 2	35	XV.	176	XXVIII.	419
III. Part 1	87	XVI.	202	XXIX.	451
III. Part 2	106	XVII.	185	XXX.	200
IV. Part 1	105	XVIII.	180	XXXI.	176
IV. Part 2	116	XIX.	187	XXXII.	207
V.	128	XX.	183	XXXIII.	212
VI.	154	XXI. Part 1	216	XXXIV.	200
VII.	178	XXI. Part 2	100	XXXV.	179
VIII.	164	XXI. (together).	94	XXXVI. (with M. N.)	263
IX.	167	XXII.	185	XXXVI. (without)	56
X.	177	XXIII.	180	XXXVII. Part 1	795
XI.	187	XXIV.	187		

The instruments belonging to the Society are as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock, No. 1,
The *Beaufoy* clock, No. 2,
The *Wollaston* telescope,
The *Lee* circle,
The *Sharpe* reflecting circle,
The *Brisbane* circle,
The *Baker* universal equatoreal.

The *Sheepshanks'* collection of instruments, viz.,—

1. 30-inch transit, by Simms, with level and two iron stands.
2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.
3. $4\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; double-image micrometer; object-glass micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters, with equatorial stand and clock movement.
4. $3\frac{1}{4}$-inch achromatic telescope, with equatorial stand; double-image micrometer; one terrestrial and three astronomical eyepieces.
5. $2\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.
6. $2\frac{1}{2}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.
7. 2-foot navy telescope.
8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.
9. Repeating theodolite, by Ertel, with folding tripod stand.
10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.
11. Portable zenith instrument, with detached micrometer and eyepiece.
12. 18-inch Borda's repeating circle, by Troughton.

13. 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms.

14. A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff; in which the telescope tube is packed; repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.

15. Level collimator, plain diaphragm.

16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.

17. Hassler's reflecting circle, by Troughton, with counterpoise stand.

18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.

19. 5-inch reflecting circle, by Lenoir.

20. Reflecting circle, by Jecker, of Paris.

21. Box sextant and 3-inch plane artificial horizon.

22. Prismatic compass.

23. Mountain barometer.

24. Prismatic compass.

25. 5-inch compass.

26. Dipping needle.

27. Intensity needle.

28. Ditto ditto.

29. Box of magnetic apparatus.

30. Hassler's reflecting circle, with artificial horizon roof.

31. Box sextant and $2\frac{1}{4}$-inch glass plane artificial horizon.

32. Plane speculum artificial horizon and stand.

33. $2\frac{1}{2}$-inch circular level horizon, by Dollond.

34. Artificial horizon roof and trough.

35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.

36. A pentagraph.

37. A noddy.

38. A small Galilean telescope, with the object lens of rock-crystal.

39. Six levels, various.

40. 18-inch celestial globe.

41. Varley stand for telescope.

42. Thermometer.

43. Telescope, with the object-glass of rock crystal.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Fuller* theodolite, to the Director of the Sydney Observatory.

The *Beaufoy* transit, to the Observatory, Kingston, Canada.

The *Sheepshanks* instrument,		No. 1, to Mr. Lassell.
Ditto	ditto	No. 2, to Mr. Huggins.
Ditto	ditto	No. 4, to Rev. C. Lowndes.
Ditto	ditto	No. 5, to Mr. Birt.
Ditto	ditto	No. 6, to Rev. J. Cape.
Ditto	ditto	No. 8, to Rev. C. Pritchard.
Ditto	ditto	No. 9, to the Director of the Sydney Observatory.
Ditto	ditto	No. 41, to Rev. C. Pritchard.
Ditto	ditto	No. 43, to Mr. Huggins.

The 6-inch circular protractor, to Mr. Birt.

The Gold Medal.

The Council have awarded the Gold Medal to M. Delaunay for his *Théorie de la Lune.*

Professor Adams will, in the unavoidable absence of the President, through ill health, explain the grounds of this award.

Printed Transactions of the Society.

Part I. of Volume XXXVII. of the *Memoirs* has been published since the last Report. This Part contains the observations of Col. Tennant on the Solar Eclipse of August 1868. The Part is illustrated by ten plates.

The papers printed in the *Memoirs* of the Society form but a small part of the contributions to the science of Astronomy which have been given to the world through our Society. In the *Monthly Notices*, already in the hands of the Fellows, will be found many papers of interest. In addition to those which form the subject of separate paragraphs of this Report may be mentioned here:—

The Society is indebted for Papers by Prof. Cayley on the attraction of Ellipsoids; on Lambert's theorem of Elliptic motion; and on the determination of a Planet's orbit from three observations.

Mr. Mann's observations of the transit of *Mercury*, Nov. 1868, and Sir T. Maclear's micrometric measures of that planet, which make its observed diameter less than the tabular diameter by 1″·50. Mr. Proctor has contributed numerous papers to our *Notices*; amongst which, one found in the March Number, on the Transit of *Venus*, 1874, will be especially referred to in another place.

Mr. Balfour Stewart read a short paper suggesting that many auroral appearances, and possibly the zodiacal light, might be caused by convection currents in the upper regions of the Earth's atmosphere, which, being conducted and moving under the lines of the Earth's magnetic force, might be supposed to be vehicles of electric currents.

Mr. Browning, besides observational papers, has described a new form of spectroscope for the telescope, and a method of using an illuminated cross for the measurement of lines in spectra.

Obituary.

The Society has to regret the loss by death of the following Fellows:—

A. K. Barclay, Esq., F.R.S.
Capt. Blake, F.R.S.
Eddowes Bowman, Esq.
E. W. Brayley, Esq., F.R.S.
J. Dickinson, Esq., F.R.S.
Prof. Donkin, F.R.S.
Chas. Mason, Esq.
Benj. Naylor, Esq.
Dr. P. M. Roget, F.R.S.
John Smith, Esq.
Sig. J. Vertu.
Rev. C. Walters.

William Fishburn Donkin was the eldest surviving son of parents remarkable for talent and excellence of character. He was born at Bishop Burton, in the county of York, on the 15th of February, 1814. Very early in life his taste for languages, mathematics, and music, became apparent, and, in addition, he possessed such perseverance and love of acquiring knowledge, that almost before he left the nursery he was known to cry when help was forced upon him which he felt he did not need, and which deprived him of the discovery he wanted to make for himself. The education he received from his father also developed a love for physics and chemistry. In 1829 his family moved to York, in order that he might have the advantage of attending a public school without losing the benefit of home influences. Here, at St. Peter's School, and under the care of Mr., afterwards Archdeacon, Creyke, he made rapid progress in classical learning. To the years spent at this school and directed in his studies by that able master, whom, as a teacher he considered perfect, William always looked back with special pleasure and gratitude.

In October 1832 he matriculated at St. Edmund's Hall, Oxford, and began to reside in the following term. On the installation of the Duke of Wellington as Chancellor of the University he recited the Greek ode. In 1834 he obtained a classical

scholarship at University College; in 1836 a first class in both classics and mathematics; and, in the following year, the Mathematical and Johnson mathematical scholarships, both of which were open to the University.

Soon after this he was elected Fellow of his own College, where he remained for the next six years, holding the post of Mathematical Lecturer, and other College offices. During these years he became known by various publications, among which may be mentioned an "Essay on the Theory of the Combination of Observations," written for the Ashmolean Society, and a very able article on Greek Music published in Dr. Smith's *Dictionary of Antiquities*.

In 1842 he was advised to stand for the Professorship of Astronomy, then vacant by the resignation of Mr. Johnson, since then Dean of Wells; and this he obtained, being at that time 28 years of age. Soon afterwards he became Fellow of the Royal Society, and also of the Royal Astronomical Society. In 1844 he married the third daughter of the Rev. John Hawtrey, at that time Incumbent of St. James's Church, Guernsey.

But even then he was beginning to suffer from a delicacy of constitution, which became a cause of great anxiety to his family; and in the midst of the most hopeful anticipations for science, when his rare abilities seemed to promise some of the discoveries which only fall to the lot of such men as he,—even then he was obliged from time to time to relinquish the duties and occupations in which he delighted, in order to try and preserve in other climates the health which was gradually leaving him. In the intervals of improvement he continued with untiring energy to attend to the duties of his Professorship, and to the advance of science in its various branches in Oxford; and during the years extending from 1850 to 1860 he sent to the Royal Society a few important papers, among which may be named one "On the Equation of Laplace's Functions," and a longer one "On a Class of Differential Equations, including those which occur in Dynamical Problems." Beyond this there is little to say of those long years during which a life that might have been one of the highest distinction was kept in check by illness and consequent inability to work. Those who knew him best, know how brightly the intellect, which otherwise might have enlightened the world, shone in the narrower sphere to which it was now confined; how his failing strength seemed only to enhance the energy that never stopped short of the limits which his state of health set to its achievements, because unable to pass beyond them; the patience that never once complained through all the weariness and comparative obscurity of his life; and the cheerfulness of character and brilliant cleverness which made a centre of happiness to his home and to his friends.

His last work, undertaken towards the close of 1866, was to prepare a book on Acoustics for the Clarendon Press series. For this he was singularly fitted, his acquaintance with practical and

theoretical music being scarcely less extensive and accurate than his knowledge of mathematics; but his increasing illness delayed the work, and the first part was still passing through the press when his friends became aware that he was about to leave them.

His death, on the 15th of November, 1869, cast a shadow over the University of Oxford, which mourned in him one who exhibited that rare combination of the highest scientific powers with the faith of a humble and sincere Christian.

The Rev. C. WALTERS came of a Welsh family, which settled first in the county of Dorset, but, about 200 years since, in Hampshire. His father, the eldest of three sons, was sent to Winchester College, under Dr. Warton (whose good opinion he won and kept), where he left a good name behind him, and made friendships which he maintained, more or less, in after life, with some who subsequently rose to rank and distinction in the University and the Church. His son, the subject of this memoir, was his only child, and he seems to have imbibed, in very early life, a taste for scientific and literary pursuits, his attention having been especially directed towards the former by his uncle, the Rev. John Vodin Walters, whom he ever regarded as a second father, and in whose memory he delighted to the last. He graduated at Oxford about the year 1808, after the usual residence at St. Mary Magdalene Hall. After his ordination he became Curate of Soberton, afterwards of Corhampton and Bishop's Waltham, then Rector of Bramdean, which he resigned for the Rectory of Wyke, near Winchester.

In regard to literary and scientific subjects, the old fire in him never ceased to burn; he gave frequent lectures at scientific institutions, and for many years was President of the Winchester Mechanics' Institute. He was elected a Fellow of this Society in 1838. He died at the advanced age of 85 years, and was buried in Winchester Cemetery.

JOHN DICKINSON, Esq., F.R.S., who died at his residence in Upper Brook Street, on the 11th January, 1869, had nearly completed his 87th year. He had for many years been engaged in the manufacture of paper, many important improvements in which were due to him. His energies were not, however, confined to manufacturing and mechanical details. He took a lively interest in all scientific pursuits, and during his lengthened life was intimate with many distinguished authors and men of science, by whom his varied information, sound sense, and conversational powers, were much appreciated. Besides being a Fellow of this Society he was a member of other learned bodies; in 1845 he was elected a Fellow of the Royal Society, to which he communicated a paper on the supply of water from the chalk stratum in the neighbourhood of London, which contains valuable information as to the amount of the rainfall in Hertfordshire, and the proportion of it which finds its way by percolation to supply the

springs in the chalk. In his latter years he took great interest in astronomy, and erected an observatory at his country house, Abbott's Hill, near Hemel Hempstead. In his astronomical pursuits he was much encouraged by his friendship with the late Admiral Smyth; but though his observatory was furnished with a fine equatoreal and a transit instrument, his advanced age prevented his making much use of them. He was well known as a liberal benefactor to numerous charitable institutions, especially to hospitals, and was for many years one of the registrars of the Royal Literary Fund. His mental and bodily activity were extraordinary, and to use the words of one of his oldest friends,—"He had thoroughly lived out his life, and had found time and means to crowd into short intervals of business more kindly and generous deeds than would make the staple of many ordinary men's lives."

Benjamin Dennison Naylor died on the 27th of December, 1868, after a few days' illness, at his residence The Knoll, near Altrincham, Cheshire. He was the last lineal male descendant of four of the clergy ejected from their livings by the Act of Uniformity in 1662. He was a governor of Chetham's Hospital and Library, Manchester, by right of descent from Humphrey Chetham the founder, who died in 1653; an institution in the management of which he took an active and benevolent interest to the end of his life. He had been many years a Fellow of the Royal Astronomical Society, and was warmly attached to all scientific pursuits, particularly to astronomy and mechanics. He took constant pleasure in his observatory, which he had fitted up with a very fine equatoreal by Cooke of York. Mr. Naylor was also an old member of the Archæological Association.

Eddowes Bowman was born in November 1810. On leaving Hazlewood School, near Birmingham (conducted by the now well-known Messrs. Hill), he entered an iron-foundry, with a view of following the engineering profession. Afterwards he undertook the management of some coal and iron works in Wales, and here the duties imposed on him were fulfilled with scrupulous exactness and to the entire satisfaction of the proprietors. A large portion of his leisure time was devoted to the moral and intellectual improvement of the people with whom he was connected, and by whom he soon became most highly respected. It was here, perhaps, that the peculiar aptitude for teaching developed itself, which characterised him through life.

After a few years thus spent, his tastes took a decided turn in favour of classical literature, and, in order to pursue his studies to greater advantage, he entered Glasgow University, where he took his degree of M.A. He then went to Germany, and studied with great earnestness under some of the most distinguished professors there.

Soon after his return to England, the chair of Greek and

Latin Classics and Greek and Roman History, in Manchester New College, became vacant by the resignation of Mr. F. W. Newman, and Mr. Bowman was then appointed to fill that office, which he did with earnestness, exactness, and thoroughness, until the College was removed to London in 1853.

From this time, without abandoning his favourite classical studies, he seems to have imbibed an increasing taste for natural science, which he pursued in its chief branches till his death, devoting much time and labour to the acquirement of the most recent knowledge in astronomy, optics, heat, electricity, acoustics, &c. These studies he pursued with his accustomed assiduity, not simply from the pleasure he derived from them himself, but that he might be better able to impart the knowledge of them to others; and, to enable him to do this more perfectly, he not only purchased the best apparatus obtainable, but spent much time and ingenuity in constructing numerous diagrams and contrivances for his lecture illustrations. His mode of lecturing was easy and fluent, and his language singularly clear and lucid.

Though astronomy was only one among a number of different branches of science cultivated by Mr. Bowman, yet his deep interest in this particular subject was evinced by the fact that, in a new residence erected some years ago, an observatory formed an important feature, and was constructed expressly to receive a new 7¼-inch equatoreal, made for him by Messrs. Cooke and Sons, of York.

It was always a pleasure to him to show this fine instrument to his friends, though his other avocations did not permit him to make so much use of it himself as either he or others could have wished.

Mr. Bowman was elected a Fellow of this Society in 1864. He died July 10, 1869.

As Signor VERTU was little known beyond the circle of his professional duties, a few words of information concerning him may prove interesting. He was born in the valleys of the Vaudois in Piedmont, and was educated at the university of Lausanne. It was there, while under the tuition of the celebrated mathematician, Emman Develey, that he began to take an interest in Astronomy; he became very fond of the science, and was possessed of some good instruments, but his time being almost absorbed by his professional duties, he sent but one contribution to the Society, which is to be found in Vol. xxiii. of the *Monthly Notices*. He died at his residence, 40 Cromston Street, Derby, on the 22nd of July, 1869, after a short illness.

Proceedings of various Observatories.

Royal Observatory, Greenwich.

The chief special work which has occupied the attention of the Astronomical Department of the Royal Observatory during the past year has been the preparation of the new Seven-year Catalogue of Stars for 1864, good progress of which has already been reported. The calculations are now completely finished, and about one-third of the Catalogue is printed; the whole work being intended to form an Appendix to the *Greenwich Observations* for 1868. We believe that its publication will be a great boon to all practical astronomers, especially as the Catalogue contains a large number of stars which have not before been observed at Greenwich since the time of Bradley.

Although the preparation and the reading of the proof-sheets of the new Seven-year Catalogue have formed the chief additional work of the Royal Observatory during the past year, absorbing necessarily a considerable portion of the computing strength; yet the ordinary observations and computations have not suffered seriously. The Sun, Moon, the principal planets, the minor planets in the first half of the lunation, the necessary stars for the determination of the clock and instrumental errors, with others whose places are required for special purposes, have all been observed with the Transit-Circle with the utmost regularity. Observations of the Moon, and of the stars and collimator, have also been daily made with the altazimuth when practicable.

The printing of the astronomical part of the volume for 1868 is in a very forward state, and the reduction of the observations made in 1869 is so far advanced that a considerable portion of the results is nearly ready for the printer.

The volume for 1868 will contain three Appendices:—1. Mr. Breen's paper, "On the Corrections of Bouvard's Elements of the Orbits of *Jupiter* and *Saturn*." 2. The Greenwich Seven-year Catalogue for 1864. 3. A Description, with Plans, of the Great Equatoreal.

Royal Observatory, Edinburgh.

The observations of fifty-five stations of the Meteorological Society of Scotland have been computed here during the last year, and published by the Registrar-General in Scotland. Time-signals by gun and ball have been daily given. Meridian observations of stars with the Transit Instrument and Mural Circle have been carried on, but not to the full extent, on account of the reduction of past observations having been taken up. A

Report to the Board of Visitors appointed by Government was published last May, and refers to several matters of exceptional interest.

Radcliffe Observatory, Oxford.

The principal change during the past year to be noted with regard to this Observatory is in its personal staff, arising from the death of the first assistant, Mr. A. Quirling. Mr. Quirling had been suffering from severe illness for several months before his death, which occurred on the morning of the 8th of June last. The vacancy which occurred in the establishment on this account, has been filled up by the appointment of Mr. Lucas to be the principal assistant, and by the engagement of Mr. S. Béchaux, B.A. of Sidney Sussex College, Cambridge.

Mr. Béchaux took first-class mathematical honours in the year 1861, and the energy and zeal with which he has devoted himself to the active duties of astronomy since his connexion with the observatory, has been already productive of the happiest effects.

The illness and death of Mr. Quirling of course produced some derangement in the business of the establishment, but not to so great an amount as might have been anticipated, as will be shown by the work which has been accomplished during the last year.

It may be mentioned, in the first place, that the volume of the Radcliffe observations for 1866, was published last midsummer, and that the astronomical portion of the volume for 1867 is nearly completed, some copies of the Catalogue of Stars (1477 in number) for that year having been circulated amongst astronomers.

The reductions of the observations for 1868 are nearly completed. The meridional observations for that year include a catalogue of about 1700 stars, 114 observations of the Sun, 63 of the Moon, 27 of *Mercury*, and 18 of *Venus*. Seven occultations of stars by the Moon were made and reduced, and the heliometer was used when the weather permitted for the observations of double stars.

For the year 1869, the transits are thoroughly reduced and ready to be made into ledgers for the formation of a catalogue of stars, in number about 1400, but only the first steps of the reduction of the North Polar distances have been performed. The number of meridional observations made during the year is quite equal to the average, the number of transits being about 3000, and of zenith distances about 4000. The usual quantity of extra-meridional work was also done in the year.

The photographic meteorology has been carried on by Mr. Lucas with the same success as heretofore, and the reductions for 1867 are nearly completed.

But the most remarkable work which has been completed during the year is the Second Radcliffe Catalogue of Stars which has very recently been finished, and some copies distributed amongst astronomers. As this Catalogue will have a separate notice in the general review of Astronomy for the past year, it will be sufficient in this place to say that it contains 2386 stars observed between the years 1854 and 1861, both inclusive.

Cambridge Observatory.

The meridian observations with the transit instrument and mural circle were made in the usual order until July 10th, when the former instrument was dismounted in preparation for the reception of the new Transit-Circle. These were chiefly confined to observations of standard stars for the determination of clock and instrumental errors, of *Mars* near opposition, and of the Sun near the equinoxes and solstices. Since July 10th, the time has been approximately found by transits taken with the telescope of the mural circle. Winnecke's comet was observed on thirteen separate nights in May and June with the Northumberland equatoreal; on each night it was compared with several stars carefully selected from catalogues. The results are found to be very accordant. Both the equatoreals have been used in observing occultations.

The meteorological observations have been made at the usual hours of 9 A.M. and 3 P.M.

The courses of a few meteors were registered in August, but the sky was unfavourable in November.

Steady progress has been made during the year with the calculations and preparations for the press; the observations of more than 1000 stars, which were made in 1864 and 1865 with the Northumberland equatoreal, by means of the square bar micrometer, for the purpose of completing the Markree zones, have been reduced, and the mean places obtained for the beginning of 1865.

The observations of Winnecke's Comet have been reduced, and the comet observations of former years are now being arranged for the press. The transit reductions are completed. The mural circle reductions are completed to the end of 1866.

All the preparations have been made for some time past for the new Transit-Circle, the arrival of which has been most unaccountably delayed, as in October last it was expected.

Glasgow Observatory.

The operations at the Glasgow Observatory during the past year have been essentially the same as those of preceding years. In addition to stars chosen from the usual list, several of the minor planets have been observed with the transit circle. The Ochtertyre equatoreal has been employed in observing such of the minor planets as may be conveniently seen with it, and do not come within the range of the meridian instrument. A few observations of Brorsen's and Winnecke's comets have also been obtained with the same instrument.

True time continues to be transmitted in the usual manner to the city and port of Glasgow. A system of meteorological observations, conducted by means of self-recording instruments, was established at the observatory in the beginning of 1868 under the auspices of the Meteorological Committee of the Royal Society, and has continued since in active operation.

Work done with the Photoheliograph at the Kew Observatory.

The first instalment of the measurements and reductions of the Kew Sun-pictures taken during the two years 1862 and 1863, containing also the areas of the observed groups and an explanation of the methods followed in the working out of the observations, has been published in the last volume of the *Transactions of the Royal Society*. Nearly 150 separate copies of the paper, printed partly at the private expense of Mr. Warren De La Rue, were distributed, chiefly to foreign observatories, scientific institutions, and distinguished astronomers and physicists.

The second instalment, containing the heliographic positions of the Sun-spots observed from the beginning of 1864 to the end of 1866, is nearly ready, and will be presented to the Royal Society at an early date during the present session.

Some investigations were also made last year on the influence which a refracting medium of considerable density would have on the apparent size and figure of the Sun, and the time of rotation, as calculated from spots at different latitudes. The preliminary discussion has led to the conviction that a comparison of the times of rotation, as derived from spots while they are near the limb, with those deduced from the same spots when near the centre, will throw much light on several important questions connected with solar physics. The matter will be exhaustively investigated in the general discussion of the Kew results.

During the present year it is intended to bring, if possible, the work of the measurements up to date. The scarcity of spots during 1867 and 1868 was such, that the pictures of these years



may be measured in a comparatively short time; and it is hoped that, by completing, by the end of this year, the observations made up to at least the end of 1869, that the greater part of the succeeding years may be devoted to as careful a discussion of the whole work as is required by the importance of the astronomical and physical problems involved in it.

Messrs. De La Rue, Stewart, and Loewy, state that the reductions of Hofrath Schwabe's observations are now finished. By comparing his observations with those taken by Carrington, and also with the Sun-pictures taken at Kew, they arrived at very favourable conclusions regarding the accuracy of the delineations of the distinguished German observer. Beginning with the year 1832, they have measured the spotted area of all his pictures up to the time when Carrington's series commenced. From the results obtained they have, first of all, deduced fortnightly views, and in the next place, in order to get rid of the more transitory fluctuations, they have taken a series of those monthly views corresponding to the middle and end of each month, from the beginning of 1832 till the end of May 1868. Putting these results into a graphical form, they have obtained a curve exhibiting only the irregularities of comparatively long periods, and from the curve by the ordinary method of equalisation, they have deduced an equalised curve exhibiting the decimal period of solar disturbances.

They find therefrom the following epochs of maximum and minimum spotted area:—

Minimum	Nov. 28	1833
Maximum	Dec. 21	1836
Minimum	Sept. 21	1843
Maximum	Nov. 14	1847
Minimum	April 21	1856
Maximum	Oct. 7	1859
Minimum	Feb. 14	1867

From these dates it will be perceived that (as has been already observed) the time between the minimum and maximum is always less than that between the maximum and next minimum.

It will also be noticed that the whole period is not always of uniform length; nevertheless, judging from what has gone before, they believe that they are perhaps entitled to conclude that the approaching minimum will not be delayed much beyond the end of this year. It ought also to be remarked that, in all the three series, the progression from maximum to minimum is not a simple progression, but exhibits in each case traces of a secondary maximum.

Finally, they have examined these results for traces of the action of the planets upon Sun-spots, and pursuing the method indicated in their preliminary researches, they derived the fol-

lowing table, as exhibiting the evidence deduced from all the observations between 1832 and 1868:—

Relative Planetary Separation.	Excess or Deficiency of Spotted Area. Jupiter and Venus	Mars and Mercury.
Between 0° and 30°	+ 881	+1675
30 ,, 60	− 60	− 139
60 ,, 90	− 452	−1665
90 ,, 120	− 579	−2355
120 ,, 150	− 705	−2318
150 ,, 280	− 759	−1604
180 ,, 210	− 893	− 481
210 ,, 240	− 752	+ 547
240 ,, 270	− 263	+ 431
270 ,, 300	+ 70	+ 228
300 ,, 330	+ 480	+1318
330 ,, 0	+1134	+2283

From this table there appears to be an excess of solar activity when either *Jupiter* and *Venus* or *Mars* and *Mercury* are together, and a deficiency when they are 180° apart. We see also that the progression of the numbers is regular in each case and very similar in the one case to what it is in the other.

Stonyhurst College Observatory.

The principal building of this Observatory stands on slightly elevated ground in the centre of the college garden. It was built in 1838 for astronomical and meteorological observations.

The ground-floor consists of a central octagon, 22 feet across, with four transepts, each 10 feet by 8 feet, at the four points of the compass. Over the centre of the octagon is a circular room, surmounted by a revolving cylindrical roof, 10 feet in diameter. This upper room was built for an achromatic by Jones, equatoreally mounted, of 4-inch clear aperture, and 5 feet 6 inches focal length. A meridian circle, by the same optician, was placed in the east transept, and another transit occupied the west transept. The diameter of Jones's meridian circle is 2 feet 6 inches; the telescope having an object-glass of 3 feet 6 inches focal length, and 3 inches aperture. Two sidereal clocks were also procured for the Observatory.

In October, 1850, the Rev. A. Weld determined very accurately the longitude of Stonyhurst by means of three of Shepherd's chronometers, the old observatory at Liverpool being chosen as the station of comparison.

Longitude of Stonyhurst	$9^m\ 52^s{\cdot}68$ W. of Greenwich.
Latitude of Stonyhurst	53° 50′ 40″ N.



In 1867 the Observatory underwent a considerable change. The 8-inch Equatoreal, which formerly belonged to Mr. Peters, was purchased by the college authorities, and a special building erected for its reception. This new observatory was placed at a considerable distance (75 yards) from the old one, for fear lest otherwise the massive iron pier on which it rests might disturb the magnets suspended in the subterranean portion of the principal building.

A circular room, 24 feet in diameter, with a revolving dome, a transit-room of 10 feet square, containing an instrument by Cary of 3 feet focal length and $2\frac{3}{4}$ inches aperture, and a study 10 feet by 12, to be devoted to spectroscopic researches, form the whole of the new observatory. The foundations of the pile for the equatoreal have been constructed with the greatest care. A bed of concrete, 10 feet square and 2 feet thick, rests on a natural foundation of coarse growing sand. On the concrete are placed four dressed stones side by side, forming a square, and each weighing more than 10 cwt. These four stones support an hexagonal column, 5 feet 6 inches in diameter, each layer of which is composed of six large cut stones, and the whole is finished with a Yorkshire landing 4 inches thick. The total height from the gravel bed to the levelling screws of the instrument is about 17 feet. Underneath the floors of the building are ceiled cellars in which a hot-water apparatus has been fitted up for protection in damp weather. The support of the telescope is an iron pier cast in two pieces; viz. a foot which weighs 13 cwt., and a pillar weighing 17 cwt. The two are fastened together by iron nuts, which allow a little play in azimuth, and the foot rests on eleven blunt levelling screws.

The object-glass, made by Troughton and Simms, has a clear aperture of 8 inches, and 11 feet 6 inches focal length. The diameter of the N.P.D. circle is 2 feet 2 inches, whilst those of the R.A. and hour-angle are each 1 foot 7 inches. The engineer who mounted the telescope was Napier, but the fixings of the N.P.D. circle are by Cary. The clockwork, which is perhaps the most perfect portion of the instrument, requires winding only every six hours. It goes excellently, and has no strain upon it, since it serves only to regulate the motion, the motive power being a weight suspended within the iron pier.

The telescope is well provided with eye-pieces and micrometers; the highest power is about 750.

The dome, which covers the instrument, runs on twelve 6-inch rollers working freely between an upper and lower surface of railway metal. The flange of the upper railway metal is bolted to the sole plate of the dome. The framework of the dome is of pitch-pine, and out of the sole plate circular ribs converge to the zenith. The frame is stiffened with a reticulation of hoop-iron, and covered with $\frac{1}{2}$-inch boards of pitch-pine. The outside is protected by a covering of painted canvass, and the internal finish is of American cloth of a dark colour. The whole

is moved by a spur wheel and pinion, gearing into a segmental rack, which rests on the wall plate. The wheel is actuated by a chain. This is so placed as to be directly opposite the shutter, and therefore generally near the observer's hand. The shutter consists merely of a frame of pitch-pine, covered outside with painted canvass, and inside with American cloth. It is worked by pulleys and bevel gearing, and its width is 4 feet 6 inches.

During the past year considerable advance has been made towards rendering the observatory practically complete. A stone pier has been erected on a brick foundation in the open air, and an equatoreal mounting attached to it. This mounting can be used for any tube, but it is principally intended for a 7½-inch Newtonian, with which the heavens can be freely swept without the necessity of moving a large dome. The two observatories have been connected by an underground wire to avoid the necessity of daily observing clock stars with two transit instruments and to have a sure check on the time at either observatory. A direct vision spectroscope has been adapted to the equatoreal by Browning. In the summer six compound prisms were procured from Hoffman of Paris, and these are being mounted for a spectroscope by Troughton and Simms. A hot-water apparatus has been arranged for the protection of the equatoreal dome and the adjacent rooms. Two observing-chairs have also been made for the large telescope. And, lastly, a new 8-inch object-glass has been ordered from Dallmeyer, the present glass not being considered sufficiently good for the excellent mounting of the instrument.

The work done has so far been mostly of a tentative character. Observations of the Moon's surface, measures of the diameters of planets and of the distances between double stars, Winnecke's comet, the November meteors, and some preliminary work with a direct-vision spectroscope, constitute the miscellaneous observations of the last twelve months. Correct time for the magnetic and meteorological self-recording instruments being a want continually felt, no fine night is allowed to pass without clock-stars being observed.

The meteorological observations, made with instruments that have been carefully compared at Greenwich or at Kew, extend in an uninterrupted series over twenty-three years.

In 1866, Stonyhurst was chosen as one of their three English meteorological stations by the Board of Trade, and was provided accordingly with self-registering barometer, thermometers, and anemometer. These instruments arrived in December 1867, and hourly readings, as also the maxima and minima of the curves traced by them, have been taken here since January 1, 1868. Both the curves and the readings are forwarded weekly to the Central Observatory at Kew.

A monthly summary of the results obtained with the meteorological instruments has been printed for private circulation during the past ten years, and a table of the reduced monthly

magnetic observations is now added to the yearly meteorological means.

The magnetic department is at present the most complete portion of the observatory. A continued series of monthly determinations of the dip, horizontal force, and declination, extend over somewhat more than six years. These have been reduced this year for the purpose of calculating the annual inequalities, &c. Previous to 1863, when a small chronometer was replaced by Frodsham's No. 3148, only occasional observations were made with the dip circle, No. 32 of Barrow, and Jones's unifilar, purchased in July, 1858; but these scattered results are of considerable use for finding the secular variation of the magnetic elements. A suitable building in a retired part of the college garden has been erected for these instruments.

During the long vacations of 1868 and 1869, a magnetic survey of the whole of France has been made with the Stonyhurst magnetic instruments, and at the expense of the college authorities. The results of the survey of the west of France, made in 1868, were presented to the Royal Society in June last, and the more numerous series of observations taken in 1869, in the east of France, are at present in process of reduction.

In 1866 a grant was made by the Royal Society towards the expense of a set of self-recording magnetographs for this observatory. In consequence, two subterranean chambers were built adjoining the old observatory. One is a photographic room, 15 feet by 12 feet. The other, a chamber 20 feet by 18 feet, in the centre of which stand the pillars supporting the three magnetographs, with two additional pillars holding telescopes, by which the observer is able to read, at any moment, the state of the magnets without interfering with the continuous photographic record. This last room is well guarded against all damp or variation of temperature. The roof is arched, with two rings of brick set in blue lias lime, and the whole covered with 6-lb. lead, and then with earth and gravel. The walls are three in number. The inner one is a single brick wall set in hydraulic lime. Surrounding this is a cavity for air 3 inches wide, with air-holes 1 foot apart, all round the room. Enclosing this is a rubble wall, 2 feet thick. And at the outside of all, as a protection from the surrounding earth, is 1 foot of loose rubble, which serves admirably for drainage. The flagged floor is built upon piers, and is thus raised 18 inches above the sand. Owing to these precautions the room keeps remarkably dry, and the temperature may be considered almost constant.

Continuous photographic records have been obtained of the declination, and of the two components, horizontal and vertical, of the intensity since January, 1868. The curves traced during the magnetic storms, which were coincident with the grand auroral displays of April and May last, were of the most perfect description, a disturbance of more than $2^{\circ}\cdot5$ in 9 minutes, having been clearly recorded in the Declination Magnetogram. Some

of the curves have been compared with those of Kew, and also with those of Signor Donati of Florence, with very interesting results.

Mr. De La Rue's Observatory.

During the past summer and autumn, numerous experiments have been made to render the luminous prominences of the Sun visible, when that luminary is not totally eclipsed. In order to effect this object, films of gold and other metals have been interposed between the 13-inch mirror and the eye-piece; and fluid coloured media have been also employed for the same object. These experiments will be again resumed during the present year.

An investigation is also being carried on, having for its object the production of a true parabolic figure, with greater certainty than has been obtainable by any of the methods hitherto used in making specula for reflecting telescopes.

Mr. Lassell's Observatory.

Mr. Lassell remarks that since his return from Malta, he has almost entirely neglected astronomy; but he has at length re-erected his two-foot equatoreal, nearly as it was at Starfield, and at Bradstones. He has chosen the form of a drum-dome, as possessing greater permanence of shape; and therefore affording greater facility of opening and shutting, without danger of the shutters being set fast by increased or unequal friction by warping. Mr. Lassell thinks that the greater capacity of the building is also advantageous, by its securing a more uniform temperature. The dome revolves on six eight-inch cannon-balls, working between cast-iron channels, the sectional curvature of which is six inches radius. The lower channel is screwed to a ring consisting of two strata of inch and a half deal plank, eleven inches broad, cut into lengths of three feet six inches, and bolted together with the end joints alternating. This ring is secured by half-inch bolts, built in the brickwork, to a nine-inch circular wall, whose interior radius is fourteen feet six inches. The upper channel is screwed to the under surface of a similarly constructed ring, of pieces of plank *three* inches thick. This forms the base ring of the dome, on which are supported three other lighter rings by intervening crosses, these lighter rings and crosses being respectively made out of inch, and inch and a quarter plank. Across the circle of the uppermost ring, but two feet six inches from its centre, is laid a trussed beam, twenty-nine feet eight inches long, which carries the side-supports of the horizontal shutter, distant four feet eight inches, and extending quite across the building. This shutter, about eighteen feet long, travels on rollers between these supports, and when withdrawn, exposes the vertical sky from the circumference of the dome to two feet six inches beyond the

centre. The roof, principally borne by the trussed beam, and having no more inclination than is necessary for carrying off the rain, is covered (as well as the circumference of the dome) with three-eighth inch deal board in two strata, which finally received a coat of well-painted canvas. The vertical shutter is framed of wood to the radius of the building, travelling on two large pulleys guided by a rail, and when opened, slides within that part of the dome which is adjacent to the opening on the preceding side, so as to be completely out of the way, and unaffected by wind, however violent.

The mode of carrying on the dome while observing, is by attaching a light pair of three-sheave blocks to the wall-plate and base-ring, the end of the rope, or fall being carried to a hook or belaying-pin attached to the observing staircase. The facilities for opening and closing the dome are by this construction eminently satisfactory, either operation being performed *in less than one minute*, without any irksome exercise of muscular power.

Mr. Lassell has also introduced a slight variation in the *mounting* of the telescope, having substituted a cast-iron frame for the mass of masonry which formerly supported the upper part of the polar axis; which renders the instrument more capable of removal, should that ever be required.

He is glad to say that the condition of the telescope on this remounting gives him the greatest satisfaction. The speculum has been laid up under a coat of varnish for more than eight years; yet on this being carefully dissolved off with a little absolute alcohol, the surface reappears with no sensible deterioration of either lustre or defining power; and Mr. Lassell sees minute points of light and the surface of *Jupiter* (barring a little abatement of his own visual acuteness) just as well as ever. He is inclined to think this size of telescope the very best in point of efficiency that can be, since it is just within the power of one observer to manage it, without being dependent on an assistant.

Mr. Lassell is afraid that he cannot promise for the future to be a very active observer; and during the few opportunities he has yet had, he has been most struck with the phase of *Jupiter*, which presents this year an aspect more distinguished by a profusion of narrow sharp belts, together with on the whole a more uniform aspect from day to day and from week to week, than used formerly to be present. On reference to a pretty numerous series of sketches of the planet which Mr. Lassell has made during more than twenty years, he is struck with the variety of configuration which has presented itself, without precisely the same form having hitherto recurred. When we reflect upon the magnitude of these colossal changes on the surface of a planet of so vast a size, we cannot help being impressed with a feeling of intense admiration, and of curiosity (if that were of any avail) to know how they are produced.

Mr. Huggins' Observatory.

In the autumn of 1868, numerous experiments were made for the purpose of finding a method by which the forms of the solar prominences could be observed directly. Several new arrangements of the spectroscope were constructed with this object in view, but at last the very simple method of employing a wide slit suggested itself, and on Feb. 13, Mr. Huggins succeeded in this way in observing *directly*, and not by inference, from the varying length of the bright lines, the outline of a solar prominence. When a spectroscope of only moderate power is employed, Mr. Huggins found it to be of advantage to limit the field of view of the little telescope to the part of the spectrum where the light of the prominence is situated, and further to interpose a red glass when the light is found to be inconveniently bright to the eye.* Other experiments were carried on for the purpose of rendering the solar prominences visible without a spectroscope, by means of the property of selective absorption possessed by many-coloured media, a method which presented itself to Mr. Huggins three or four years since. The problem to be solved was to find a combination of media which would absorb light of all refrangibilities except precisely that of C., or of F. A large number of mineral and vegetable substances was examined; the most promising combination which has been found consists of a solution of carmine in ammonia which cuts off nearly all the light more refrangible than C., combined with a solution of chlorophyll which gives a band of absorption, taking away the brighter part of the light less refrangible than C. As yet chlorophyll has only been obtained in a state in which the band of absorption encroaches upon C., and weakens the light of the prominence.†

The observations in this observatory were interrupted in the autumn by the necessary preparations to fit the building for the reception of a refractor of 15 inches aperture, which is being constructed by Mr. Grubb of Dublin, for the Royal Society, by whom it will be placed in the hands of Mr. Huggins.

* *Proceedings Royal Society*, vol. xxvii. p. 302.
† *Monthly Notices*, vol. xxx. p. 36.

Notes on some Points connected with the Progress of Astronomy during the Past Year.

Discovery of Minor Planets and Comets.

In the last Annual Report a record was made of the discovery of the unprecedented number of twelve asteroids during the year 1868. In this respect, the planetary additions in 1869 contrast very unfavourably with those of the preceding year. The general cloudy state of the sky during the last twelve months has interfered sensibly with delicate astronomical observations of all kinds, and none more so than with those of the minor planets and similar faint objects. However, during occasional intervals of clear weather, two of these minute bodies have been added to the asteroid list; (108) *Hecuba*, discovered on the 2nd of April 1869, by Dr. R. Luther, at Bilk; and (109) *Felicitas*, on the 9th of October, by Dr. C. H. F. Peters, at Hamilton College, Clinton, New York.

Several of the minor planets discovered in 1868 have received their distinctive names since the date of the last Report, a record of which may be useful in this place. They are (99) *Dike*, (103) *Hera*, (104) *Clymene*, (105) *Artemis*, and (106) *Dione*.

Three telescopic comets have been detected during the past year. The most important is Comet I. 1869, known also as Winnecke's periodical comet. This object had been identified at a previous perihelion passage with a comet discovered by Pons in 1819. According to a recent paper by M. Oppolzer, inserted in the *Astronomische Nachrichten*, we gather that there are very strong grounds for believing that it is also identical with a comet discovered by M. Pons so far back as 1808. M. Oppolzer, however, hopes to be able to speak more definitely on this point when he has completed some additional investigations on the comet's orbit, on which he is now engaged.

The two remaining comets — Comet II. 1869, and Comet III. 1869 — were both discovered by M. Tempel, of Marseilles, the first on October 11, and the second on November 27. Several observations have been made of each at different observatories and approximate elements of their orbits have been computed.

Proposed Prizes for the Discovery of Comets.

The Fellows will be glad to learn that the *Imperial Academy of Sciences at Vienna*, acknowledging the importance of continuous cometary investigations, especially in connexion with

observed streams of meteors, has proposed to give a series of prizes, limited to eight annually, for the discovery of telescopic comets during the three years' interval between May 31, 1869, and May 31, 1872. The prizes will be awarded at the annual meeting of the Academy, which usually takes place at the end of May, and will consist either of a sum of money,—twenty Austrian ducats—or of a gold medal of equivalent value. In a circular distributed among astronomers, the Academy has published a series of rules for the guidance of the discoverers. From its principal clauses, it appears that the comet must be invisible to the naked eye at the time of discovery, and also one whose appearance could not have been predicted with any degree of certainty. Notice of the discovery must be immediately sent to the Academy at Vienna either by post or telegraph, without waiting for further observations. The message must, however, always be transmitted by telegraph when practicable. The authorities of the Academy undertake to communicate the discovery immediately to the principal observatories. The prize will not be awarded to any discoverer of a comet which has not been observed by another astronomer unless his first record of the comet's place is supplemented by further observations, sufficient in number for the determination of the elements of the orbit.

The Council are glad to hear that the *Imperial Academy of Sciences at Vienna* has thus so generously determined to encourage a systematical searching for telescopic comets, a branch of astronomical discovery which has been comparatively neglected of late years. They trust that the initiative taken by the Vienna Academy will be heartily responded to, not only by the Continental astronomers, but by the Fellows of this Society, many of whom are now so well furnished with the necessary instruments for cometary observations.

Division of Labour in Computing Ephemerides of the Minor Planets.

Observers of the minor planets are now practically dependent upon the *Berliner Jahrbuch* for the annual ephemerides of these objects. The rapid increase in their number renders it necessary to make a distribution of labour among computers, to allow of proper provision being made for the computation of the tabular places. The Fellows may, therefore, be interested to know by whom the important work which appears in the pages of the *Berliner Jahrbuch* is accomplished. The following list gives the names of the computers, and the planets for whose tabular places for 1870 each is responsible :—

M. Adolf	Helena, Mnemosyne.
Dr. Albrecht	Sappho.
Dr. Anderson	Undina.

Dr. Auwers	Circe.
Dr. Von Asten	Cybele, Diana, Egeria.
Dr. Becker	Amphitrite, Beatrix, Clotho, Erato, Eugenia, Europa, Hygeia, Niobe.
Dr. Bruhns	Bellona, Irene.
Dr. Brunn	Isis.
Dr. Celoria	Clytie.
Dr. Deike	Thisbe.
Dr. Dunér	Panopea.
Dr. Engelmann	Eurydice.
Dr. Gunther	Ægina, Calliope, Calypso, Daphne, Euterpe, Galatea, Lætitia, Massilia, Phocea, Urania.
Mr. Hall	Terpsichore.
M. Hoek	Proserpine.
M. Karlinsky	Hestia.
Dr. Kowalczyk	Hesperia.
Dr. Krüger	Themis.
M. Lehmann	Eunomia, Metis, Minerva, Vesta, Victoria.
M. Leppig	Aurora, Flora.
M. Leveau	Hera.
M. Liegel	Lutetia, Pomona.
Dr. Lorek	Semele.
Dr. R. Luther	Danaë, Hebe, Melete, Parthenope.
M. Möller	Pandora.
Nautical Almanac	Pallas
Dr. Oppolzer	Angelina, Concordia, Olympia.
Dr. Peters (Altona)	Sylvia.
Dr. Peters (Clinton)	Echo, Feronia, Frigga, Ianthe, Io, Miriam.
Dr. Powalky	Aglaia, Doris, Fortuna, Freia, Harmonia, Nysa, Pales, Virginia.
M. Prey	Ariadne.
Mr. Safford	Alcmene.
Dr. Schultz	Alexandra.
Dr. Schur	Arethusa, Melpomene.
M. Sievers	Atalanta, Leucothea, Polyhymnia, Psyche, Thalia.
M. Stark	Hecate.
Dr. Stolz	Asia.
Dr. Tiele	Fides.
Dr. Tietjen	Ausonia, Leda, Nemausa.
Dr. Tischler	Eurynome.
Dr. Valentiner	Clio.
M. Vogel	Ægle, Antiope.
M. Westphal	Euphrosyne.
M. Wolfers	Ceres.
M. Wolff	Julia, Leto.

The Total Solar Eclipse of August, 1869.

This eclipse was successfully photographed in America, by two of the eclipse parties organised, on the proposal of Prof. Coffin, superintendent of the *Nautical Almanac* of the United States, by Prof. H. Morton, of Philadelphia, who took charge of these expeditions. It was also photographed by Commander Ashe, director of the observatory at Quebec.

The apparatus employed by the American astronomers was similar in principle, but of larger optical power, than that used by De La Rue in Spain, in 1860, that is to say, the principal image was enlarged by a Huyghenian eye-piece before it reached the sensitive plates. The adoption of this form of apparatus was decided upon "after a careful study of De La Rue's reports and pictures, as also those of the late expeditions," which led Prof. Morton to the conclusion that the plan of enlargement presented many advantages over that of making the photograph directly in the principal focus of the telescope; one of these advantages consists in the superior definition of the position-wires over that which obtains when they are situated within the focus of the telescope, as in the form of instrument used by Major Tennant. Certain modifications in the form of the Huyghenian eye-piece were made by Mr. Zentmayer, in order to adapt it specially for the photographic work, and the instantaneous slide was also somewhat different from that used by De La Rue. The telescopes employed in taking the photographs possessed a great advantage in respect of aperture over the Kew Heliograph, being 6 inches in diameter, whereas the object-glass of the Kew instrument is only 3·4 inches in diameter; moreover, in the instruments used in the American expeditions, the primary image of the Sun is enlarged only to about two inches in diameter, whereas in the Kew Heliograph the image is enlarged to 3·8 inches, so that during totality, when the full aperture of the objective was employed as in both cases in the Kew Heliograph, the area-ratio of the objective to that of the picture was as 0·8 to 1, whereas in the American telescope it was as 9 to 1, or about 11·25 times greater. Six pictures were taken during the totality at Burlington, 40° 48′ 17″ N.L., and $0^h\ 56^m\ 14^s$ west of Washington; and four at Ottumwa, situated about 75 miles west of Burlington, by another party. The totality pictures at Burlington were obtained with exposures of from 5 to 7 seconds, and those at Ottumwa in from to 6 to 16 seconds. In 1860, De La Rue's first picture was obtained with an exposure of 60 seconds; on taking into consideration the relation of aperture to size of picture, the Burlington pictures would represent an exposure of 56·25 to 78·75 seconds, and the Ottumwa 67·5 to 180 seconds in the Kew Heliograph. It does not, therefore, appear that any step has been made in the sensitiveness of

the chemicals since 1860, or that the luminosity of the protuberances greatly differed in 1869 from that in 1860.

The American pictures are very perfect specimens of astronomical photography; and have the advantage from the relative shortness of the exposure of showing the state of the phenomena in reference to a given epoch with more precision than was possible in 1860. By comparing together the photographs obtained at Burlington and Ottumwa, it is seen that the protuberances are absolutely identical in both series except in so far as they are more or less covered in consequence of the parallactic displacement of the lunar disc. They also show that those brilliant surroundings of the Sun, not bounded by such distinct outlines as the protuberances, and yet whose forms are clearly distinguishable, are identical in both series of the photographs. This goes to confirm the photographic observations of De La Rue in 1860, and Tennant and Vogel in 1868, showing that the protuberances properly so called do not constitute the sole surroundings of the Sun's photosphere, but that a portion at least of the coronal brightness belongs also to the Sun. On the occasion of this eclipse, it was noticed by Prof. Pickering, who accompanied one of Prof. Morton's parties to Mount Pleasant, that, while "the sky was strongly polarised all around, close up to the corona, that object itself was not a source of polarised light." It will be recollected, on the contrary, that during the total eclipse of 1868, it was observed both in Egypt and India that the light of the corona was strongly polarised. On future occasions this apparent discrepancy will undoubtedly receive such attention as it may be hoped will lead to a true solution.

Several photographs of the partial phase were obtained with beautiful definition. At Burlington, a record of the first contact was procured; and at Ottumwa, a picture at the last instant, just before totality, which gives a photographic record of the phenomenon known as Baily's beads, being, as Prof. Morton remarks, "simply the last glimpse of the Sun's edge cut by the peaks of lunar mountains into irregular spots." The pictures of the partial phase all show an increase of light on the solar surface in contiguity with the limb of the Moon, as was observed by De La Rue in 1860. Prof. Morton was at first inclined to attribute this to the existence of a lunar atmosphere; but subsequent experiments have led him to conclude that the cause is entirely chemical, and does not correspond to any celestial phenomenon; an analogous appearance is frequently to be seen in terrestrial photographs. It has been suggested by Mr. Stone, that there is a possibility of its being at least partially due to the scattering of the Sun's light which falls on the Moon's edge at a grazing angle, and that part of the light scattered is superadded to the direct light of the Sun near the Moon's limb.

Commander Ashe obtained four photographs of the totality at Jefferson Town, with an 8-inch equatoreal, the pictures being taken in the principal focus of the instrument. These pictures,

although they do not possess the sharpness of the American photographs, confirm what has already been stated in regard to the identity of form preserved by the protuberances and the entities with soft outlines; unfortunately in photographs 3 and 4, there is evidence of the disturbance of the telescope during the exposure of the sensitive plate.

Atmospheric Dispersion.

The Astronomer Royal has employed his great theoretical and practical knowledge of optics to correct the unpleasant, and for delicate observations, fatal effects of atmospheric chromatic dispersion at low altitudes. By providing prisms of different angles the mean effects of the chromatic dispersion can be entirely destroyed. In the *Monthly Notices* for January, an ingenious and simple method of applying a prism of varying angle has been described. It is certain that with eye-pieces of this construction, the difficulties in making delicate observations at low altitudes will be diminished.

Spectrum Analysis.

Our Fellow, Mr. Lockyer, has pursued with great diligence and success his spectroscopic researches on the Sun. Since the last Report he has found that several other substances besides hydrogen are occasionally to be detected above the photosphere, namely, sodium, barium, magnesium, and iron. The presence of the bright lines of these substances rising above the photosphere, and associated with the bright lines of hydrogen, Mr. Lockyer regards as indicating a state of disturbance of the solar matter, greater than was present at the time of his earlier observations. From a slight alteration in refrangibility of the bright lines of hydrogen, as shown by their want of perfect coincidence with the corresponding lines of absorption, Mr. Lockyer believes that he has evidence of rapid currents in the solar matter, sometimes attaining a velocity of 40 miles per second in a vertical direction, and a velocity of 120 miles per second in a horizontal or cyclonic direction. He considers that his observations support the conclusion that "the chromosphere and the photosphere form the true atmosphere of the Sun, and that, under ordinary circumstances, the absorption is continuous from the top of the chromosphere to the bottom of the photosphere." In these observations Mr. Lockyer is accumulating a store of facts, upon which we may hope to base a more complete theory of the constitution of the Sun than we now possess.

In the early part of last year Mr. Huggins, by the simple method of using a wide slit, afforded a means of observing directly the forms of the prominences.

Zöllner has proposed a new form of spectroscope, in which the

light of a star is divided between two prisms, each half of the light being refracted in an opposite direction. By this means small differences in position of stellar and solar lines appear doubled in amount, and can be observed, therefore, and measured with great accuracy. Zöllner, by using a wide slit, has made careful observations of the forms of the solar prominences, and of the rapid changes which take place in them. His observations confirm the conclusions as to the very great mobility of the matter of the red flames which had been previously arrived at by Janssen and Lockyer from the rapid changes they had observed in the bright lines of these objects.

Mr. Plummer observed successfully the spectrum of the Aurora Borealis in April 1869. His observations agree with the previous spectroscopic examination of this object by Angström and Struve.

Spectroscopic observations of the Aurora have also been made by Prof. Winlock.

Father Secchi has continued his spectroscopic researches on the Sun and stars. They are described in the *Comptes Rendus.*

Lunar Radiation.

The thermo-electric pile has been employed by more than one observer during the past year in experiments upon lunar radiation, and with some success. By means of the "three-foot" Parsonstown reflector, a thermopile, and a Thomson's reflecting galvanometer, a second thermopile being also connected with the galvanometer to compensate for disturbing effects, the Earl of Rosse succeeded, during the lunations of March and April 1869, in obtaining evidence of lunar heat increasing in amount with the phase of illumination. The manifested heat-rays were, however, shown to be principally obscure; and Lord Rosse hence infers that the greater part of the heat received from the Moon consists of solar heat that has been first absorbed by the lunar crust, and then given off in dark radiation. No evidence of cosmical heat was obtained. By comparison of the solar and lunar actions upon the pile, a vessel of hot water being used as an intermediate standard of reference, it was estimated that the relation of solar to full-Moon radiation is as 89819 to 1, a proportion sensibly the same as that arrived at by a purely theoretical investigation. By the use of a cistern of hot water, with a blackened exterior, exposed to the pile under similar circumstances to the Moon-lit surface of the great mirror, Lord Rosse found that the amount of deviation which the full Moon caused in his galvanometer appeared to indicate an elevation of temperature of the lunar surface through 500° Fahrenheit.

Some later observations have been made upon the same subject in Paris, respectively by M. Baille, at the Ecole Polytechnique, and M. Marié-Davy at the Paris Observatory. The

former employed a concave mirror of 39 centimetres aperture to condense the Moon's rays upon his pile, and also made use of a Thomson's galvanometer. The one conclusion at which he arrived was, that the full Moon, at Paris and in the summer months, gave as much heat to his pile as a radiating surface 6·5 centimetres square, maintained at boiling-water temperature and placed at a distance of 35 metres. M. Marié-Davy has published results of two series of measures secured during the lunations of October and November last. The first were made with a pile attached to a 9-inch equatoreal refractor, the second with an 8-inch mirror; the object-glass in the former case having been found to intercept a large proportion of the heat-rays. M. Marié-Davy's measures confirm those of Lord Rosse. They show that the heating effect of the Moon increases with the illumination of the visible disk. Between October 9, when the Moon was four days old, and October 20, when it was full, the measured heat of the condensed beam of moonlight increased from 0°·00017 (Centigrade) to 0°·00287. If this last number be divided by the ratio of the area of the concentrated image to the area of the object-glass, we have twelve-millionths of a centigrade degree as the direct heating power of the full Moon at the Earth's surface. This is the result given by the object-glass: that afforded by the mirror is about six times as great. It will be seen that M. Marié-Davy has converted his galvanometer indications into centigrade equivalents: how this conversion was effected, and how the constancy of the scale indications is secured, if it is secured, we are not informed. He confirms Lord Rosse's inference that the proportion of solar to lunar radiation is about as 80000 to 1, and likewise concludes that the Moon imparts to us no heat from an internal or cosmical source. Further, he infers that the diffusive power of the lunar surface is considerable, at least equal to that of the least coloured of terrestrial rocks; and he finds that the lunar heat by reason of its large percentage of obscure rays is far more impressionable by atmospheric humidity than that from the Sun.

It will be remembered that Professor Smyth, in his Teneriffe experiments, determined the heating power of the full Moon to be equal to one third of that of a Price's candle at a distance of 14 feet 9 inches. M. Marié-Davy finds that such a candle at such a distance affects his pile to the extent of 0°·00075 centigrade, which he conceives to be the heating power of the Moon upon the summit of Teneriffe, upon the supposition that the heat emitted by the respective candle-flames was sensibly the same.

Heating Power of the Stars.

The experiments upon this subject, commenced by Mr. Stone with the great Equatoreal of the Greenwich Observatory, in the year 1868, and mentioned in the last Annual Report, were continued

as weather permitted during 1869. In pursuing them, Mr. Stone was led to the construction of a thermopile which there is good reason to believe will prove of value in thermometric researches other than those of the class for which it was specially prepared. His early trials convinced him that it was almost impossible to distinguish the feeble currents generated by stellar heat from the grosser effects produced upon the pile by exposure of one face within the telescope tube and the protection of the other face outside the telescope tube. It was evident that to maintain the pile in thermal equilibrium its two faces must be exposed to precisely similar atmospheric influences. Mr. Stone therefore resorted to what in effect may be described as a horse-shoe pile, the two faces of which being similarly presented to the object-glass of the telescope were affected alike by disturbing causes, whether these took the form of draughts of air or cooling of the metals of the pile by radiation. By this arrangement the whole heat of a star's image cast upon either face of the pile manifested itself *per se*. Decided indications of heat from *Arcturus* and *α Lyræ* were thus obtained on several nights. The amounts were measured by a reflecting galvanometer; and by a somewhat tedious process the galvanometer indications were converted into Fahrenheit-scale equivalents. It was found that the heating effect of *Arcturus*, after allowing for absorption by the object-glass, was 0·00000137 of a Fahrenheit degree; that of *α Lyræ* being about two-thirds of this amount. Otherwise expressed, the heat from *Arcturus*, at an altitude of 25° at Greenwich, is about equal to that from a three-inch cube of boiling water at a distance of 400 yards, while from *α Lyræ* it is equal to that from the same cube at 600 yards. Mr. Stone conceives that the difference of heating power may be connected in cause with difference of colour. He finds that the manifested heat diminishes rapidly as the amount of moisture in the air increases, and that all sensible effect is cut off by the slightest cloud or haze. The details of the investigation are published in the *Proceedings of the Royal Society* for January, 1870.

Transit of Venus, 1874.

The preparatory arrangements for the observations of the transit of *Venus*, 1874, are sufficiently advanced to encourage us to expect most important additions to the data already collected for the determination of our fundamental astronomical unit of length. The British Government has already placed at the disposal of the Astronomer Royal sufficient funds for the equipment of five stations, each with an Altazimuth and Transit, for the determination of longitude and local time, and with two telescopes, one of which is to be of six inches aperture, and to be

provided with driving power. Four of the larger instruments have already been obtained, and the Transits and Altazimuths are in actual progress in the manufactory of Messrs. Troughton and Simms. The second telescope at each station will be of 4 inches aperture.

The present intention appears to be to place the British observers at Kerguelen's Island, Woahoo, Auckland in New Zealand, Alexandria, and Rodriguez.

A Commission, consisting of Admiral Paris, MM. Faye, Laugier, Villarceau, and Puiseux, has reported to the Bureau des Longitudes that it would be particularly desirable for the French astronomers to occupy the Islands of Saint Paul and Amsterdam, Yokohama, Tahiti, Nouméa, Mascate and Suez.

The North German astronomers have referred the consideration of the action which they should urge upon their Government, to a Committee, of whom the illustrious astronomer of Gotha was elected chairman.

This Committee appeared to lay great stress upon the employment of heliometers for fixing the relative position of *Venus* on the Sun at the different stations.

Equatoreals with driving power for the eye-observers were to be of 6 feet focal length and 52 lines aperture. The employment of photography and spectroscopic observations was discussed. The chief objection to the employment of photography appeared to be in the question of expense, although Professor Argelander was not satisfied with the degree of accuracy which might be expected from it. The further consideration of this question was, however, deferred to a sub-committee. Spectroscopic observations were only proposed to indicate the approach of the planet for the observation of external contacts. The Committee recommended that the Government be urged to fit out four expeditions,— two to the North and two to the South. The stations specially referred to as favourable were, Nertschensk, Hohodadi, Kerguelen, Edwards', Crozet's, and Auckland Islands, and in certain respects Mauritius.

The Russian territories offer most valuable positions for the location of observers. These stations are certain to be well occupied by the Russian astronomers. The Director of the Imperial Observatory at Pulkova has already secured a Committee for the consideration of a proposition to establish a chain of observers across the country from Kamtschatka to the Black Sea, at intervals of about 100 miles. This appears desirable on account of uncertainties connected with the atmospheric conditions in the month of December.

It is to be hoped that, for the same reason, there will not be a too great crowding of the observers towards one or two points, in other regions, to the exclusion of others of nearly equal importance; and that, in this matter at least, after the best consideration has been given to the subject, we may all adopt instruments of not very unequal power, and attempt to make the same class of

observations in the same way. Uniformity in these observations means success; want of uniformity, comparative failure.

We would here recall attention to the beautiful series of maps by Mr. Proctor of the parts of the Earth turned towards the Sun at the principal phases of the Transit, which accompany his paper in the March Number. This paper contains a careful deduction from Mr. Hind's results in the *Comptes Rendus*, vol. liii. p. 131, of the chief stations for the observations of internal contact, deduced with direct and especial reference to that phase as the most important point of observation. On account of the nature of the motion of the planet relatively to the Sun, this exhibits in a more striking light than had previously been done, the value of Halley's method in the Transit of 1874.

Re-observations of the Stars down to the 9th magnitude in Argelander's Zones from N.P.D. 10° to 92°.

Dr. Auwers has lately distributed a catalogue of 539 stars to be used in connexion with the proposed re-observation on the meridian of stars within the zones of N.P.D. 10° and 92° down to the 9th magnitude. This re-observation has been recommended by the German Astronomical Society. The scheme states that sufficient accuracy will be obtained by two observations of each star. Each observation to be taken over three or four wires, and the circle readings to be made with two microscopes. The work has been distributed amongst thirteen observatories. One zone, that from N.P.D. 35° to 40°, alone remains unappropriated.

34 *Messier.*

Dr. O. A. L. Pihl has lately republished with additions his paper on this cluster which originally appeared in the Supplementary *Monthly Notices*, for 1868. With an equatoreal of only 4¼ inches aperture, he has found an interesting and useful field of investigation by laying down the relative positions of 85 stars in this cluster. The positions are fundamentally referred to two principal stars as zero points, and very considerable accuracy appears to have been obtained. A re-examination of this cluster at some future time, and a comparison with Dr. Pihl's positions, may lead to important results.

The Newton-Pascal Forgeries.

"L'Examen de la Discussion soulevée au sein de l'Académie, par M. Leverrier," contains a complete exposure of the falsehood of the Newton-Pascal papers which M. Chasles was induced by the skill of an artful forger to communicate in such profusion

to the French Academy. M. Leverrier deserves great praise for the combination of talent, scientific skill, and zeal in the cause of truth which proved so efficient in finally destroying this imposture. However, the first indisputable astronomical proofs of the falsity of these documents are contained in the two letters which Prof. Grant of Glasgow addressed to M. Leverrier on September 12, 1867, and October 31, 1867, and which were published without delay in the *Comptes Rendus*.

Prof. Grant's objection of the identity of the so-called results of the masses, &c. of the planets with the corresponding results contained in the third edition of the *Principia*, was unanswerable.

We may thus assert that, in conjunction with that of his distinguished friend, Sir David Brewster, the name of our Fellow, Prof. Grant, will ever remain most honourably connected with the exposure of the Newton-Pascal imposture.

The Great Newall Telescope.

The 25-inch equatoreal telescope, commenced several years ago by T. Cooke & Sons, of York, for R. S. Newall, Esq., of Gateshead, is now so far completed that it has been removed from the works at York into its observatory in Mr. Newall's grounds, at Ferndene.

The general design and appearance is the same as that of Cooke's well-known equatoreals on the German principle improved; but the extraordinary size of all the parts has necessitated the special arrangement of most of them.

The length of the tube, which is of riveted steel plates, including dew-cap and eye-end, is 32 feet, and it is of cigar shape; the diameter of the object-end being 27 inches, and the centre of the tube 34 inches. The cast-iron pillar supporting the whole is 19 feet in height from the ground to the centre of the declination axis, when horizontal; and the base of it is 5 feet 9 inches in diameter. The trough for the polar axis weighs alone 24 cwt.: the weight of the whole instrument being nearly 9 tons.

The object-glass has an effective aperture of 25 inches (nearly). In order as much as possible to avoid flexure from the unequal pressure of the object-glass, it is made to rest upon three fixed points in its cell, and between each of these points are arranged three levers and counterpoises round a counter-cell, which act through the cell direct on to the glass, so that its weight in all positions is almost equally distributed among the 12 points of support, a slight excess being thrown upon the 3 fixed ones. The focal length of the object-glass is 29 feet. A Barlow lens is arranged to slide on a brass framework within the tube. The hand is passed through an opening in the side of the tube, and by means of a handle attached to the cell the lens may be pushed into or out of the cone of rays.

Attached to the eye-end of the tube are two finders, each

having an object-glass of 4 inches aperture; they are fixed above and below the eye-end of the main tube, so that one may be readily accessible in all positions of the instrument. It is also supplied with a telescope having an object-glass of 6½ inches aperture. This is fixed between the two finders, and is for the purpose of assisting in the observations of comets and other objects for which the large instrument is not so suitable. This assistant telescope is provided with a rough position circle and micrometer eye-pieces, and is illuminated by Cooke's new illuminating apparatus.

The driving clock is in the upper part of the pillar, and is of comparatively small proportions, the instrument being so nicely counterpoised that a very slight power is required to be exerted by the clock, through the tangent screw, on the driving wheel (7 feet diameter), in order to give the necessary equatoreal motion. The clock is regulated by a pendulum, the intermittent motion of which is converted into a continuous one by the peculiar and ingenious arrangement of a double train of wheels, invented and exhibited to the Society by the late Mr. T. Cooke; with the exception that a fan, revolving in an air-box, the sides of which are perforated with holes at regular intervals, alternately opened or closed by the action of the clock as it goes faster or slower, is substituted for the double spring which acted as a brake, a most elegant contrivance, first, it is believed, employed by the late M. Foucault.

The declination axis is of peculiar construction, necessitated by the weight of the tubes and their fittings, and corresponding counterpoises on the other end, tending to cause flexure of the axis. This difficulty is entirely overcome by making the axis hollow, and passing a strong iron lever through it, having its fulcrum immediately over the bearing of the axis near the main tube, and acting upon a strong iron plate rigidly fixed as near the centre of the tube as possible, clear of the cone of rays. This lever, taking nearly the whole weight of the tubes, &c., off the axis, frees it from all liability to bend.

The hour-circle on the bottom of the polar axis is 26 inches in diameter, and is divided on the edge,* and read roughly from the floor by means of a small diagonal telescope attached to the pillar; a rough motion in right ascension by hand is also arranged for by a system of cog-wheels moved by a grooved wheel and endless cord at the lower end of the polar axis, so as to enable the observer to set the instrument approximately in right ascension by the aid of the diagonal telescope.

The declination and hour-circles will be illuminated by Geisler's tubes, and the dark and bright field illuminations for the micrometers will be effected by the same means.

Mr. Norman Lockyer, who had an opportunity of examining

* The hour-circle is also divided on the face, and read by micrometer microscopes.

and trying this magnificent instrument shortly before its despatch to its owner Mr. Newall, speaks in the highest terms of its mounting and mechanical arrangements generally; and so far as he was able to judge of its optical performance, with the low power to which the state of the weather restricted him, he considered it to be very promising.

The difficulties attending the constructing and mounting of so large an object-glass are exceedingly great. That all these then have been entirely overcome no one conversant with the refinements involved can reasonably expect, and the optical and mechanical perfection of the famous Cooke factory has raised the standard by which telescopes are judged. We have every hope that this instrument will be worthy of their fame.

This record of enlightened private enterprise would be incomplete if we did not add that Mr. Newall proposes, when the instrument is properly fixed in a good climate, to place it at the disposal, for a certain number of hours in each day, of any qualified astronomer who desires to use it. The benefit likely to be rendered to science by Mr. Newall's liberality can hardly be overrated. We understand that his observatory is to be under the direction of Mr. Albert Marth. We have not yet heard that its site has been finally decided on; it is certain that the full powers of such a telescope cannot be called forth in the climate of England.

Mr. Buckingham's Telescope.

In connexion with this splendid accession to observational astronomy, we would call attention to the existence of a telescope not much inferior in size to that just described, which was constructed some years ago by Mr. J. Buckingham, a Fellow of this Society, and which was exhibited by him at the International Exhibition of 1862. Its clear aperture is 21¼ inches, and its focal length 28½ feet. It is mounted as an equatoreal of the (so called) German form. Mr. Buckingham has also constructed another telescope of 9 inches aperture, mounted equatoreally. Competent judges, who have been permitted to visit Mr. Buckingham's Observatory at Walworth, speak in the highest terms of the completeness and ingenuity of the arrangements. It is understood that Mr. Buckingham has employed a method, peculiar to himself, of practically correcting the aberrations of the large object-glass. Regarding its optical performance we have as yet no definite report. Mr. Buckingham is known to be not only a mechanician of great originality, but also an industrious observer. It is very much to be desired that he should find leisure from his arduous occupations to give to the world a com-

plete account of his optical processes, of the contrivances peculiar to his instrumental mountings, and of the performance and produce of the magnificent apparatus which he has created with so much labour.

Zenith Sectors for Indian Survey.

One of the zenith Sectors constructed for the Great Trigonometrical Survey of India, by Messrs. Troughton and Simms, from Colonel Strange's designs, has been despatched, and has reached its destination, Bangalore, in the Madras Presidency. It went out by the Overland route under charge of Lieut. Rogers, R.E., an officer of the Survey, and is said to have sustained no injury in transit. This instrument has a telescope of 4 inches aperture and 4 feet focal length. The sectors are portions of a circle 3 feet in diameter; they are read by four micrometer microscopes, illuminated by a simple light somewhat in the manner employed in the transit-circle of the Royal Observatory. It is furnished with the appliances of a zenith telescope, and can be used, therefore, as such if desired. The instrument is to be employed in the observation of latitudes on the southern portion of the great meridional arc of India at short intervals, with the twofold object of investigating the very interesting physical question of local attraction, and of eliminating the effects of such attraction on the geodetic determination of the figure of the Earth. These operations have been intrusted to Capt. J. Herschel, R.E.

Mr. Carrington's Observatory.

All who are interested in Astronomy will be glad to hear that our Fellow, Mr. Carrington, whose valuable observations of circumpolar stars and of Sun-spots are well known, is now engaged in erecting a new observatory at Churt, near Farnham. This observatory is arranged on a new plan of construction. As it is situated on a conical hill, entirely detached, and 60 feet high, no elevation of the building is needed. Mr. Carrington has sunk the observatory below ground, so that the instruments can just be directed over the soil at the top of the mound. There has been sunk a dry well, 6 feet in diameter, to a depth of 40 feet, in which a clock, placed in an air-tight case, may be kept at a position of invariable temperature and at a constant and diminished pressure. The observatory will be furnished with an Altazimuth on Steinheil's principle, in which the horizontal axis is also the optical axis; an object-glass of 6 inches, provided with a prism fixed outside it, is placed at one end, and the eye-piece at the other end of the axis.

We hope this observatory, when it is completed, will speedily be as well known for the valuable contributions to astronomy proceeding from it as Mr. Carrington's former observatory at Redhill.

Second Radcliffe Catalogue of Stars.

We have to record the publication by the Radcliffe Observer of a second catalogue consisting of 2386 stars, deduced from observations extending from 1854 to 1861. The observations were made with the transit instrument and the meridian circle, by Jones, the use of which has been discontinued since the establishment of the Carrington circle in 1861. This circumstance made it desirable to embody in a distinct catalogue the observations made with those instruments.

The stars contained in the Catalogue have been, in general, observed many times, and their places may therefore be considered to be fixed wiih the greatest accuracy. Mr. Main has instituted a most careful examination between the right ascensions of the principal stars and those given in the Greenwich Seven-year Catalogue for 1860. From this comparison corrections have been deduced and applied to the Radcliffe results, in order to reduce this Catalogue to the same fundamental epoch as that of the Greenwich Catalogue. A similar comparison has also been made between the resulting north polar distances of these catalogues, and a slight systematic discordance appears between the results which has been attributed, with great probability, to outstanding uncorrected errors of division in the circles.

The magnitudes assigned to the stars are in most cases deduced from observation, the number of estimates on which the final result being based is given in a separate column.

In the introduction it is stated that the whole of the work connected with the formation of the Catalogue has been made by Mr. Main and one assistant Mr. Luff.

Pulkova Observations.

The observations made with the Transit Instrument of the Pulkova Observatory, from 1842 to 1853, have been printed in two magnificent volumes, and distributed for the use of astronomers. The first volume contains a full and most interesting Introduction by the illustrious Director, Mr. Otto Struve, giving an account of the methods adopted and observations made for the determination and elimination of the errors of the instrument and of its position. In the same volume there is a Catalogue of the Mean Right Ascensions of 348 Stars reduced to the epoch 1845, with star-constants for two epochs, 1840 and 1870.

Chinese Astronomy.

The Assistant-Secretary, Mr. Williams, has presented to the Society a MS. work, entitled, *Comets observed in China from* B.C. 613 *to* A.D. 1640. These observations have been extracted from Chinese works of authority, and are given with the original text. They are translated and arranged chronologically, with a full explanation of the several asterisms, described as occurring in the path of the comets. They are preceded by some introductory remarks in which the progress of Chinese astronomy is traced from between two and three thousand years before the Christian era to the present time. There is also an Appendix, containing chronological and other tables, required in the reduction of Chinese time to our reckoning, the construction and use of which tables are fully explained in the Introductory remarks, with numerous examples of their application. To facilitate the identification of the asterisms mentioned in the observations, a Chinese celestial atlas has been constructed, traced from an original work on astronomy. In this the thirty-one divisions into which the Chinese divide the visible heavens are laid down separately with the stars composing them, according to our nomenclature.

Communications to the Society from February 1869 *to February* 1870.

1869.

Mar. 12. Observations of the Transit of *Mercury*, 1868. Sir T. Maclear.

Ditto ditto Mr. Abbott.

Ditto ditto Mr. Barneby.

Occultations observed at Liverpool. Mr. Joynson.

On the Solar Eclipse of August 7, 1869. Mr. Hind.

On the Aden and Guntoor Photographs of Eclipse, 1868. Mr. De La Rue.

Note on the Attraction of Ellipsoids. Mr. Cayley.

On the Meteoric Shower, Nov. 1868. Sir T. Maclear.

On the Transit of *Venus*, 1874. Mr. Proctor.

Rotation of *Mars*. Mr. Proctor.

On an extensive Train of Sun-spots. Mr. Browning.

On an Improved Mode of Mounting Finders. Mr. Browning.

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A Method of constructing Charts by which in a few moments the great circle course between any two points may be accurately ascertained. Mr. Proctor.

1870. A New Theory of the Milky Way. Mr. Proctor.

Jan 14. Observations of Meteors, Nov. 13, 1869, in America. Mr. Davidson.
Observations of *Jupiter's* Satellites. Mr. Joynson.
On a Bright Cross Micrometer for measuring the position of lines in faint Spectra. Mr. Browning.
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Seventh Catalogue of Double Stars observed at Slough, 1823–1829 inclusive, 84 of which had not been previously described. Sir John Herschel.

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ADDRESS

Delivered by the Chairman, Professor J. C. Adams, on Presenting the Gold Medal of the Society to M. Charles Delaunay.

Gentlemen,—It has been announced to you that the Society's Medal has been awarded to M. Ch. Delaunay for his great work on the Theory of the Moon.

The illness of our excellent President having made it impossible for him to be present on this occasion, the Council have done me the honour to request that I would occupy the chair, and in his stead lay before you the grounds of their award. I have acceded to their wishes with the more readiness because I have given some attention to special branches of the Lunar Theory, and my study of M. Delaunay's work has led me to form the highest opinion of its merits.

Of all the problems presented to us by physical astronomy none has so much engaged the attention of mathematicians as that of the determination of the motion of our satellite. The theoretical interest as well as the great practical importance of the results, has proved an irresistible attraction, and the mathematical difficulties have merely acted as a stimulus to the invention of various methods of surmounting them. It is fortunate that this has been the case, as the excessive labour involved in any theory of the Moon approaching to completeness, might otherwise have proved too great for human perseverance. The foundations of the theory were laid by Newton in his *Principia;* and although his investigations are only fragmentary, being simply intended to show



how some of the leading lunar inequalities may be deduced from theory, yet they form one of the most admirable portions of that immortal work. Towards the middle of the eighteenth century the theory was more systematically entered upon by Clairaut, D'Alembert, and Euler, who severally showed that the theory was competent to give very approximate values of all the inequalities which were then recognised by observation.

Still the theory was far from being sufficiently perfect to serve as a foundation for lunar tables accurate enough for the uses of navigation. This degree of accuracy was first attained by the tables of Mayer, who not only carried the approximations to the values of the coefficients of the various lunar inequalities further than his predecessors had done, but also corrected the theoretical coefficients thus obtained by comparison with his own observations. The theory was greatly advanced by Laplace, not only by his more accurate theoretical determination of the coefficients, but also by several important discoveries, especially that of the cause of the Moon's secular acceleration.

The improvements in the lunar tables, however, which were made successively by Bürg and Burckhardt, were founded, not on theory, but on comparison of the former tables with observations; and the empirical tables thus produced were far more accurate than any that could have been formed at that time by theory alone. Dissatisfied with this state of things, and wishing to see astronomy founded exclusively on the law of attraction, only borrowing from observation the necessary data, Laplace induced the Academy of Sciences to propose for the subject of the mathematical prize which it was to award in 1820 the formation, by theory alone, of lunar tables as exact as those which had been constructed by theory and observation combined. The prize was divided between two memoirs — one by M. Damoiseau, the other being the joint production of MM. Plana and Carlini. Damoiseau's memoir is printed in the third volume of the *Recueil des Savants Etrangers*. Plana's great work on the lunar theory, which appeared in 1832, is the development of the joint memoir by himself and Carlini. By these important works an immense advance was made in the theory, the approximations being carried to such an extent that the resulting coefficients were comparable in accuracy with those given by observation. In 1824 Damoiseau published tables founded entirely on his theory, which were found to be quite as exact as those of Burckhardt.

Both Damoiseau and Plana, following the example of Laplace, start from differential equations in which the Moon's longitude is taken as the independent variable; and after the equations have been integrated, they obtain the values of the Moon's co-ordinates in terms of the time by reversion of series. An important innovation, however, was introduced by Plana in the mode of conducting the investigation and exhibiting the results. The values of the Moon's co-ordinates being developed in series of sines and cosines of angles which vary uniformly with

the time, the coefficients of the several terms of these series will depend on the eccentricities of the orbits of the Sun and Moon, the inclination of the Moon's orbit to the plane of the ecliptic, the ratio of the mean motions of the Sun and Moon, and the ratio of their mean distances from the Earth. Now Damoiseau, in common with all previous writers, having assumed certain values of the quantities just mentioned as given by observation, contented himself with determining the numerical values of the coefficients. Although this is all that is required for the construction of tables, yet, from a theoretical point of view, it leaves the mind unsatisfied, inasmuch as any coefficient in its numerical form shows no trace of its composition, that is of the manner in which its value depends on the value of the assumed elements. The several coefficients are far too complicated functions of the elements to be represented analytically, except in the form of infinite series, and Plana, accordingly, developes these coefficients in such series, proceeding by powers and products of the eccentricities, the tangent of the inclination, the ratio of the Sun's mean motion to that of the Moon, and the ratio of the Moon's mean distance to that of the Sun, all these quantities being assumed to be small, and the last mentioned ratio, which is much smaller than the others, being considered as a quantity of the second order.

In this mode of development, the numerical factor which enters into any term of the coefficient of any of the lunar inequalities is an ordinary fraction which admits of being determined not merely approximately, but with absolute accuracy. It is easy to see what great facilities are afforded by this circumstance for the verification of the work by a comparison of the results obtained by different methods. The greater or less degree of approximation will thus depend on the greater or less number of terms taken into account in the several series.

The numerical values of the several elements are not substituted in the formulæ until the work is completed, and this is attended with the important advantage that when a comparison of the theory with observation has supplied more accurate values of the elements, their corrected values can be at once substituted in the same formulæ, without requiring any additional work.

On the other hand, if the numerical values of the elements be introduced into the calculations from the first, then if it is desired to introduce corrected values of the elements, much additional investigation will be required for the purpose.

No doubt the labour required in order to obtain a given amount of numerical accuracy by this method is very much greater than is required when each coefficient, instead of consisting of a series of terms, is reduced to a simple numerical quantity, but the great theoretical advantage of knowing the composition of every coefficient in terms of the elements well repays the additional labour.

The degree of convergence of the series obtained for the

several coefficients is in general sufficiently rapid, but in some few of the coefficients, on the contrary, the convergence is so slow, at least in the leading terms, that it is necessary to take into account terms which are analytically of a higher order than those to which the approximation is in general limited.

Thus Plana, who proposed to himself to determine the lunar inequalities completely to the fifth order, found it necessary in special cases to carry the approximation to the seventh and even to the eighth order, and in several cases he also added an estimated value of the remainder of the series founded on the observed law of diminution of the calculated terms.

Soon after the publication of Plana's great work, Sir John Lubbock formed the plan, which he partly carried out in his various tracts on the theory of the Moon, of verifying Plana's results by a totally different method, starting from differential equations in which the time is taken as the independent variable, and thus avoiding the necessity of reversion of series.

Later, M. de Pontécoulant undertook the same work on a similar plan, and carried it out more completely in the fourth volume of his *Théorie Analytique de Système du Monde*.

These works, while they corrected some errors which had crept into Plana's computations, confirmed their wonderful general accuracy, and with some few exceptions they do not extend the approximation beyond the order to which Plana restricts himself.

Meantime, M. Hansen had undertaken a completely new investigation of the lunar theory, by a remarkable method peculiar to himself and explained in his *Fundamenta nova investigationis orbitæ veræ quam Luna perlustrat*, which appeared in 1838.

In applying the method described in this work to the case of the Moon, M. Hansen throughout employs numerical values of the elements of the Moon's orbit, and consequently the coefficients of the lunar inequalities as obtained by him are also purely numerical. The process is one of successive approximations, which are repeated again and again until the values of the inequalities which are found from the last approximation sensibly coincide with those which were assumed in entering upon that approximation.

The numerical values of the coefficients thus finally obtained are undoubtedly very exact. The slight corrections which these coefficients still require are probably chiefly due to the small corrections required by the numerical elements on which the calculations are based, and in the method employed no provision is made for taking into account the effect of these corrections.

From his formulæ, M. Hansen constructed tables of the Moon, which were published in 1857, at the expense of the British Government; and these tables, having been found far superior in accuracy to all others, are now exclusively employed in the calculation of ephemerides.

A detailed account of the calculations leading to M. Hansen's

last approximation, was given by him in the two parts of hi *Darlegung der Theoretischen Berechnung der in den Mondta feln angewandten Störungen*, which severally appeared in 186 and 1864.

After the great works, to which we have thus briefly referred had been either completed or were in progress, it might hav been supposed that the matter was exhausted.

Our Associate M. Delaunay, however, was not of this opinion Having devised, so long ago as 1846, a perfectly original and singularly beautiful method of integrating the differential equations of the Moon's motion, he determined to apply this method to the complete re-investigation of the theory, and to carry on the approximation to a much greater extent than had been done by his predecessors. The principal fruits of his labours, to which he has devoted himself with almost unexampled perseverance for so many years, are contained in the magnificent volumes which the Imperial Academy of Sciences have done both M. Delaunay and themselves the honour of publishing among the volumes of their *Memoirs*. It is for this great work that your Council have awarded to M. Delaunay the Society's medal.

Strongly impressed with the advantages of determining the co-efficients of the lunar inequalities in the analytical form, both as affording a solution more complete in itself and more satisfactory to the mind, as well as one offering facilities for the comparison of the results of different investigations, M. Delaunay did not hesitate to follow the example set in this respect by M. Plana, notwithstanding the immense length of the necessary calculations. M. Delaunay's results are thus obtained in a form which makes them directly comparable with those of M. Plana, while the methods employed in obtaining them are wholly different.

M. Delaunay chooses the *time* as the independent variable, and takes as his starting-point the differential equations furnished by the theory of the variation of the arbitrary constants. In an able Memoir which appeared in 1833, Poisson had advocated the employment of these equations in the theory of the Moon's motion, and he applied them to the discussion of some special points of that theory. These equations had been long used, almost exclusively, for the determination of the perturbations of the planets, and they offer peculiar advantages in the treatment of the secular inequalities and those of long period. In the case of the Moon, however, in consequence of the large perturbations caused by the disturbing force of the Sun, the ordinary mode of integrating these equations by successive approximations soon leads into calculations of inextricable complexity. In fact, these equations give the differential coefficients of the several elliptic elements, taken with respect to the time, in terms of the elements themselves. In the case of the planets, where the disturbing forces are so small compared with the predominant central force of the Sun, very approximate values of the disturbed elements may be found by substituting in the values of the differential coefficients,

the undisturbed instead of the disturbed values of the elements, and then integrating.

The perturbations of the elements thus found are said to be due to the first power of the disturbing force. If now the approximate values of the disturbed elements be substituted in the differential equations, and these be again integrated, we shall obtain a second approximation to the values of the disturbed elements, and the additional terms thus found are said to depend on the square of the disturbing force. In the theories of the planets it is only in special cases that terms depending on the square of the disturbing force need be taken into account, and it is scarcely ever necessary to consider terms of the next order of approximation.

In the case of the Moon, however, it would be necessary to repeat the process of approximation at least four or five times, in order to obtain results of the accuracy required in the present state of the theory. If we consider that the disturbing function consists of a great number of terms; and that each term gives rise to a corresponding term in the value of each of the disturbed elements, while powers and products of the corrections of all the elements in every possible combination, up to a certain order, have to be taken into account, it may be readily imagined how impracticable it would be by such a process to carry on the approximation to a greater extent than has been already done by Plana. Every process in which the approximations require to be repeated several times, is subject to the inconveniences that have been described, and these inconveniences are much greater when, as in the present case, we have to make successive approximations to the values of the *six* elements of the orbit, instead of to the values of the *three* co-ordinates of the Moon.

It was with the view of avoiding this excessive complication of the method of successive approximations that M. Delaunay devised his method of integrating the differential equations of the Moon's motion. The fundamental idea of this method consists in attacking the difficulty by small portions at a time, and in replacing these extremely complicated successive approximations by a much greater number of distinct operations, each of which is comparatively simple, so that it may be carried out to any degree of exactness that may be desirable, while the mind is relieved by being able readily to embrace the whole of each operation in one view.

It is difficult, without the use of algebraical symbols to give an idea of M. Delaunay's beautiful method, but I must endeavour, in some measure, to fulfil this task, and I must crave your indulgence should I fail in the attempt.

The theory of the variation of the arbitrary constants gives, as is well known, the differential co-efficients of the elliptic elements with respect to the time, in terms of the elements themselves and the partial differential co-efficients of a certain function, called the Disturbing Function, taken with respect to those ele-

ments. By a proper choice of elements, the differential equations may be reduced to their simplest, or to what is called their *canonical* form. In this form the six elements are divided into three pairs, the elements of each pair being conjugate to each other. Then the differential coefficient of any element with respect to the time is simply equal to the partial differential coefficient of the disturbing function taken with respect to the element which is conjugate to the former, the partial differential coefficients which occur in the two equations corresponding to the pair of conjugate elements being affected with opposite signs.

The disturbing function may be readily developed in a series of periodic terms involving cosines of angles, each of which is formed by the combination of multiples of the Moon's mean longitude, the distance of the Moon's perigee from its node, and the longitude of the node, together with angles which depend on the position of the disturbing bodies. The disturbing function likewise contains a non-periodic term, which, as well as the coefficients of the periodic terms, are all functions of the major semi-axis, the eccentricity and the inclination of the Moon's orbit.

Since the mean longitude of the Moon involves the time multiplied by the mean motion which is a function of one of the elements, it is obvious that the differentiation with respect to this element will give rise to terms in which the time occurs without its being included under a sine or a cosine. Such terms would render the equations very inconvenient for the determination of the lunar inequalities; and M. Delaunay accordingly avoids the introduction of them by taking the mean longitude itself instead of the *epoch* of mean longitude, as one of his elements, while by the simple yet novel expedient of adding to the disturbing function a non-periodic term which is a function of the major semi-axis alone and is independent of the disturbing forces, he preserves to the differential equations the same very simple form which they had at first. After this modification of the disturbing function, the time no longer enters into it explicitly except in so far as it is introduced by the values of the co-ordinates of the disturbing bodies, and consequently the difficulty which was before met with completely disappears.

The six elements employed by M. Delaunay are thus,—the Moon's mean longitude, the distance of the perigee of its orbit from the node, and the longitude of the node, which for distinction may be called the three *angular* elements, and three other elements which are respectively conjugate to the former, and which are determinate functions of the major semi-axis, the eccentricity and the inclination of the orbit.

The three co-ordinates of the Moon at any time are given in terms of the three angular elements and of the quantities last mentioned.

Now let us imagine, for a moment, that the disturbing function contained no periodic terms, but was reduced simply to its non-periodic part. Consequently the partial differential co-

efficients taken with respect to the angular elements would all vanish, and therefore the three conjugate elements would be all constant, as well as the major semi-axis, the eccentricity and inclination, of which those elements are functions. Hence, again, the partial differential co-efficients taken with respect to the conjugate elements would be functions of those elements, and would therefore be constant. Hence each of the angular elements would consist of an arbitrary constant and a term proportional to the time, the multiplier of the time in each case being a known function of the three constant elements.

The object of M. Delaunay's method is, by means of a series of changes of the variables, to cause all the more important periodic terms to disappear from the disturbing function, one by one, while the differential equations continue to retain their canonical form, so that after each transformation we approach more nearly to the conditions of the ideal case which has just been considered.

In order to effect any one of these transformations, M. Delaunay supposes, for the moment, that the disturbing function is reduced to its non-periodic part, together with one of the periodic terms selected from among those which have the greatest influence in producing the lunar inequalities. With this simplified form of the disturbing function, the equations admit of being easily integrated. The elements with which we start may thus be expressed in terms of three new angular elements which vary uniformly with the time, and three new constant elements. M. Delaunay shows how the constant elements may be so chosen that they may be considered as respectively conjugate to the three new angular elements, so that, in fact, the quantities which are multiplied by the time in the expressions of these angular elements are respectively equal to the partial differential co-efficients of a function of the new constant elements taken with respect to these elements.

Having thus found the relations between the old set of elements and the new ones by means of the simplified form of the disturbing function, M. Delaunay now restores the complete value of that function, and chooses new elements which are connected with the old ones by exactly the same relations as in the case just considered. Of course the three new angular elements will no longer vary uniformly with the time, and the three elements respectively conjugate to these will no longer be constant.

When, by means of the proper formulæ of transformation, the new variables have been substituted for the old ones in the disturbing function and in the expressions of the Moon's co-ordinates, M. Delaunay shows that—

1st. One of the important terms of the disturbing function disappears, viz., the periodic term which was selected in the preliminary investigation.

2nd. Various inequalities corresponding to this term are introduced into the values of the three co-ordinates of the Moon.

3rd. The values of the six new variables in terms of the time

are determined by differential equations of exactly the same form as those which determined the values of the six variables for which they have been substituted.

One of the periodic terms having been in this manner caused to disappear from the disturbing function, a new operation of exactly the same kind causes another term of this function to disappear; similarly a third term may be taken away by means of a third operation, and so on to any number of terms.

In this way, after a suitable number of operations of this kind have been effected, the disturbing function will have been simplified by the removal from it of its most important periodic terms, after which the further process of integration becomes simple enough to be treated in the same manner as if we were concerned with the perturbations of a planet or of the Sun.

The whole difficulty in the determination of the lunar inequalities is caused by the great magnitude of the disturbing force of the Sun. M. Delaunay has therefore at first confined his attention to the investigation of the irregularities which are produced by this disturbing force, and the two magnificent volumes before us are entirely occupied with this investigation. Thus he has provisionally left out of consideration the very small inequalities due to some secondary causes, such as the attraction of the planets and the figure of the Earth; and, besides, he has omitted to consider the perturbations of the Sun's apparent motion about the Earth, intending in a supplementary volume to take into account the effects due to these several causes.

By means of repeated applications of the beautiful method of transformation which I have above attempted to describe, M. Delaunay proceeds to get rid of all the periodic terms of the disturbing function due to the Sun's disturbing force, which are capable of producing inequalities in the co-ordinates of the Moon of an order inferior to the fourth. For this purpose fifty-seven such operations are required to be performed. When these have been effected, the periodic terms which remain in the disturbing function are so small that their powers and products may be neglected, and consequently the differential equations which determine the six elements last introduced in terms of the time, may be integrated at once. Since the values of the Moon's co-ordinates are known in terms of the elements just mentioned and the time, we have only to substitute the values of the elements that have been found, in order to determine the Moon's co-ordinates in terms of the time.

The values of the elements, however, that would be found in this way are very complicated, and therefore the substitutions which would be required in order to find the Moon's co-ordinates would be excessively long. M. Delaunay, accordingly, prefers to get rid of the remaining periodic terms in the disturbing function, one by one, by means of transformations exactly similar to those which have been already effected. In order to carry on the approximation to the extent which he desires, M. Delaunay finds it necessary to perform no less than 448 of these secondary

operations, but each such operation becomes very simple, since the squares of the coefficients of the periodic terms under consideration may be neglected.

Thus, at length, by means of 505 transformations, all the periodic terms of the disturbing function are removed, and the problem is reduced to the ideal case which was considered at the outset of our account of M. Delaunay's method.

After each transformation, by making the proper substitutions in the expressions for the Moon's co-ordinates, those co-ordinates are obtained in terms of the system of elements last introduced, so that finally the three co-ordinates are known in terms of the three final constants and angles which vary uniformly with the time.

It has been already mentioned that Plana, in his great work on the Lunar Theory, determined the analytical values of the coefficients of the lunar inequalities as far as terms of the fifth order inclusive, and that he only carried on the development to a greater extent in cases where the slowness of the convergence of the series appeared to him to render it necessary to take into account terms of higher orders than the fifth.

M. Delaunay has proposed to himself to carry on the approximation so as to include all terms of the seventh order, and in cases where the series converge slowly to take into account terms of the eighth, and even of the ninth order.

Those who have had any experience in calculations of this nature will readily understand how enormously the labour required has been increased by thus adding two orders more to those which Plana has considered. It is not merely that the terms of higher orders are far more numerous than those of the lower, but also that each of the terms of the former kind is much more difficult to calculate, since it arises from a much greater number of combinations of terms of the inferior orders.

This enormous labour, which has occupied M. Delaunay for nearly twenty years, has been performed by him without assistance from any one. Indeed, from the nature of the calculations which are required, it would not have been easy to obtain any effective assistance. In order to insure accuracy, M. Delaunay has omitted no means of verification, and he has performed all the calculations, without exception, at two separate times, with a sufficient interval between them to prevent any special risk of committing the same error twice in succession.

The volumes before us are perfect models of orderly arrangement. Notwithstanding the great length and complication of the calculations, the whole work is so disposed that any part of it may be specially examined with the utmost readiness by any one who may wish to test its accuracy.

Finally, the analytical expressions which have been obtained for the Moon's co-ordinates are converted into numbers, by substituting for the elements the most accurate numerical values which the comparison of theory with observation has made known.

Such is an imperfect sketch of M. Delaunay's labours on the

Theory of the Moon contained in these two magnificent volumes, the former of which appeared in 1860, and the latter in 1867. As I have already stated, they do not include a complete theory of the Moon, but only that which is by far the most difficult and complicated part of that theory, viz., the investigation of the perturbations due to the direct action of the Sun supposing its apparent motion about the Earth to be purely elliptic. Of the investigations which are required to take into account the remaining very small causes of disturbance, and which are intended by M. Delaunay to be included in a supplementary volume, some of the most important have been already completed by him, particularly the calculation of the Secular Variation of the Moon's Mean Motion, and the investigation of the long inequalities due to the action of *Venus*.

I understand also that M. Delaunay is engaged in the construction of new Lunar Tables founded upon his theory.

Your Council, however, has decided that we ought not to await the appearance of M. Delaunay's supplementary researches before we mark emphatically our sense of the value of his labours.

The present work is complete in itself; in it the very difficult and complicated problem of determining the Moon's motion is attacked by a perfectly original method, and that one as powerful and beautiful as it is new. The work has been planned with admirable skill and has been carried out with matchless perseverance. The result is an enduring scientific monument of which our age may well be proud, and which we are happy to distinguish, on this occasion of our fiftieth anniversary, with the highest marks of our approval which it is in our power to bestow.

The Chairman, then delivering the Medal to M. Delaunay, addressed him in the following terms:—

M. Delaunay, il ne me reste plus maintenant qu'à vous présenter cette médaille au nom de la Société Royale Astronomique, qui désire par ce tribut vous exprimer la haute appréciation qu'elle a de vos travaux. Notre Président regrette vivement que l'état de sa santé l'empêche de remplir cette tâche agréable. Il m'a prié de le remplacer dans cette circonstance, et je le fais avec d'autant plus de plaisir que depuis bien long-temps j'ai la plus grande estime pour vos hauts talents, et que j'ai étudié vos belles recherches avec la plus grande admiration, aussi je suis heureux de vous exprimer que notre Société vous a suivi dans votre immense travail avec le plus vif intérêt; et quoique ce travail ne soit pas entièrement terminé, elle sent qu'elle ne peut tarder plus long temps à reconnaître la haute valeur de vos recherches. Nous sommes heureux de vous voir au milieu de nous à cette occasion, et nous faisons des vœux pour que votre santé et vos forces puissent durer de longues années afin d'enrichir la science de plus en plus du fruit de vos grands talents.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

WILLIAM LASSELL, Esq. F.R.S.

Vice-Presidents:

G. B. AIRY, M.A. F.R.S., Astronomer Royal.
WARREN DE LA RUE, Ph. D. F.R.S.
Rev. ROBERT MAIN, M.A. F.R.S., Radcliffe Observer.
Admiral R. H. MANNERS.

Treasurer:

SAMUEL CHARLES WHITBREAD, Esq., F.R.S.

Secretaries:

WILLIAM HUGGINS, Esq. F.R.S.
EDWARD J. STONE, Esq. M.A. F.R.S.

Foreign Secretary:

Lieut.-Col. ALEXANDER STRANGE, F.R.S

Council:

J. C. ADAMS, M.A. F.R.S., Lowndean Professor of Astronomy, Cambridge.
JOHN BROWNING, Esq.
THOMAS BURR, Esq.
A. CAYLEY, M.A. F.R.S., Sadlerian Professor of Geometry, Cambridge.
JOHN HENRY DALLMEYER, Esq.
E. B. DENISON, Esq. M.A. LL.D.
EDWIN DUNKIN, Esq.
GEORGE KNOTT, Esq.
J. NORMAN LOCKYER, Esq. F.R.S.
Captain WILLIAM NOBLE.
Rev. CHARLES PRITCHARD, M.A. F.R.S., Savilian Professor of Astronomy, Oxford.
BALFOUR STEWART, Esq. M.A. LL.D. F.R.S.

Dome for Sale.

Captain Swann, the present proprietor of Redhill Observatory, wishes to part with the dome, which was put up by Mr. Carrington at an expense of more than 150*l.* He will take 50*l.* for it; it is in as good condition as when first erected. It is of iron throughout, 25 feet diameter.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *March* 11, 1870. No. 5.

WILLIAM LASSELL, Esq., President, in the Chair.

Charles Meldrum, Esq., Government Observatory, Mauritius; and
Rev. H. C. Watson, Trinity College, Cambridge,

were balloted for and duly elected Fellows of the Society.

Dark Objects Crossing the Sun's Disk.

An unusual phenomenon was noticed by Lieut. Herschel while observing the Sun at Bangalore, in India, on the 17th and 18th of October last, the following particulars of which are extracted from a letter to his brother, Prof. A. S. Herschel, dated Bangalore, October 20–25th, 1869. At about noon on the 17th, while preparing to observe the red prominences of the Sun, with an equatoreal refractor of 5 inches aperture armed with a spectroscope, Lieut. Herschel first threw the Sun's image on a sheet of white cardboard, placed as a screen, to obtain a general view of any spots which might be visible on its disk. Some dark shadows were soon noticed crossing the Sun, and afterwards some light streaks beyond its border. The first were attributed to birds, and the second to sparks inside the tube; but their frequency first, and then their uniformity of direction attracted consideration, as evidently indicating that an unusual phenomenon was in progress, and a few minutes' attention showed that what were dark shadows on the Sun were luminous moving images beyond its border. The possibility of the passage of a meteoric stream having here suggested itself, the Sun's image was sketched, and a pencil was drawn across it wherever a shadow passed. In ten minutes

thirty or more lines were drawn, and their accordant direction proved that it was really a continuous stream.

The clock-work was now adjusted, and a friend's assistance was obtained to move the screen, so as to keep the Sun's image fixed upon it by means of the positions of images of conspicuous Sun spots; while, whenever a shadow appeared a ruler was placed, and a line was instantly drawn in the direction which it had taken. After about ten minutes the observers changed places, and thus secured two diagrams containing forty-four and thirty-eight lines respectively, and noted the N. and S. direction by shifting the image of a solar spot in declination. The apparent sizes of the shadows were defined by three marks upon the margins of the diagrams. A similar image of the Sun being next cast with the large finder of the equatoreal, it was found that the shadows crossed that as well. On diligently looking through the finder, and also through the main tube of the telescope, the images were at length visible there, not as shadows crossing the Sun's disk, but as ill-defined passing sparks near the Sun's border. They were thus seen and repeatedly examined by both of the observers until the Sun set.

At seven o'clock on the following morning the appearances were the same, the bodies still passing in a continuous stream. Fresh drawings were made; and it was found possible to obtain views of them in the spectroscope. Soon after noon on the 18th, the following principal features, or apparent characters of the bodies, were recorded. "1. Their direction is towards about 150° E. of North, but it is almost certain that there are two streams. 2. They are not very distant. The majority of them are completely out of focus when the Sun is in focus. When focus was adjusted on a passing cloud they all appeared much better defined. 3. They are brightest near the Sun, as well as most frequent; in spite of the overpowering tendency of the Sun's light they distinctly lose their brightness as they leave the Sun, and acquire it as they approach. 4. They vary greatly in size and velocity, and in distinctness of definition. As a rule the smaller they are the slower they move, and the more distinct is their form; but there are exceptions. The slower ones can be followed up, by casting loose, and may be traced several degrees from the Sun. 5. Their motion is exceeding irregular; not to the extent, however, of *in the least degree* making their average direction and velocity uncertain, but only in comparison with that regularity which is to be expected in cosmical matter. One only out of hundreds (if it was one) was retrograde in direction. Not unfrequently, however, their path is contorted or devious. On one occasion (the most marked instance that I can recollect) one entered the field slowly in the usual direction, but on reaching the centre seemed to meet a transverse current, with which it was swept away at right angles to its former course. On the whole the motion resembled that of floating particles, subject to the influence of a mingling of many currents. 6. Their number is

anything short of infinity. Fifteen or twenty in a minute were repeatedly counted across the field of view, 45′ in diameter. 7. Their form is very difficult to describe. For a long time the impression was as of a half-moon moving diameter forwards, or sometimes edgeways. Then the feeling was that there were large luminous snowflakes of various sizes, the smaller ones being almost stellar in their distinctness and brilliancy. But since I tried focussing on a distant cloud they seemed with one accord to take a tangible and real shape, a kind of double crescent with a bar across, and wings or phantom-like appendages accompanying, thus—  Whatever was their shape, it had reference to the direction of their motion, and not to the Sun. 8. Their spectrum is solar. Vivid flashes from top to bottom are seen as they cross the slit."

Later in the afternoon, having by continued effort managed to see them without reflecting eye-piece or dark-glass almost up to the very edge of the Sun, with a power of 55, Lieut. Herschel almost satisfied himself that that shape which he had seen so distinctly was a delusion, and that the real shape was a disk; when light clouds, passing over, at first interrupted a perfect view of the objects, but eventually solved the question of their nature. The particles streaming by in regular direction were intensely brilliant, and many of them moved so slowly as to take many seconds crossing the field of view.

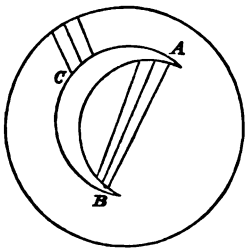
At last one of the objects *paused*, *hovered*, and whisked off, and in that instant the observer writes that he saw—  "There was no longer any doubt; they were locusts, or flies of some kind. The next morning (October 19th) they were still streaming by in hundreds in the same direction; but I paid little attention to them now, but put up the spectroscope to look at prominences. After a while they were passing the slit so frequently that I might have counted twenty or thirty in a minute. It remains to be seen if the appearances will continue. As it was, the continuous flight for two whole days, in such numbers, in the upper regions of the air, of beasts that left no stragglers is a wonder of natural history, if not of astronomy."

At the time when the above description was written, the *Homeward Mail* contained the news that countless locusts had descended upon certain parts of India. An appeal has also recently been made in the daily newspapers, stating that a famine has arisen at Jerusalem on account of the destruction of every green herb there by the devastations of innumerable locusts.

Among the appearances which may, perhaps, receive a partial explanation from the above observations of Lieut. Herschel, are some very similar phenomena recorded by the observers of the total solar eclipse on the 7–8th of August last at some of the stations in the United States of America.* At Ottumwa, "about

* *Journal of the Franklin Institute*, for 1869, p. 200, *et seq.*

twenty-five minutes before totality, Prof. Zentmayer observed some bright objects on the ground glass, crossing from one cusp to the other of the solar crescent, as indicated by the accompanying cut by the lines from A to B; each object occupied about two seconds in passing, and they all moved in right lines, nearly parallel, and in the same direction. These points were well defined, and whatever they might be, must (in order to produce such sharply defined images on the ground glass) have been several miles distant from the telescope. After calling Prof. Himes' attention to this phenomenon, and observing some eight or ten bodies in all, Mr. Zentmayer then noticed three others coming in from the limit of the field, and disappearing in the solar crescent, as shown at C; but not reappearing on the other side. It is worthy of note that the direction of motion of the three bodies last mentioned coincided with that of the wind blowing at the time, but that of the others did not; and they are thus, as also for other reasons, unlike the plant seed noticed some years ago by Mr. Dawes."* Prof. Coffin also saw "meteoric bodies cross the telescope from east to west like bright flakes."†



The eclipse having taken place very near the periodical date of the 10th of August, it is interesting to remark that at Vevay, Indiana, during the eclipse, several meteors were seen, at an altitude of about 45°, taking a westerly direction. On the other hand the bright objects seen near the Sun's disk by Prof. Zentmayer, and the bright flakes noticed by Prof. Coffin, were not impossibly caused by the distant passage between the observer and the Sun's disk of some winged tribe as the extraordinary flight of locusts, seen through the telescope by Lieut. Herschel, in India, in October last.

Notes on the Solar Corona and the Zodiacal Light; with suggestions respecting Observations to be made on the Total Solar Eclipse of December 24th, 1870. By Richard A. Proctor, B.A.

The total eclipse of next December will last so short a time that, if possible, no part of that time should be wasted through a misapprehension of the nature of the phenomena to be observed. On this account I cannot but think it would be a matter to be much regretted that mistaken views should be promulgated respecting the corona, supposing it to be possible,—which I take

* *Monthly Notices*, for 1852, p. 183.
† *Journal of the Franklin Institute*, for 1869, p. 152.

to be the case,—to form just views from the evidence already in our hands.

The principal object of the observations to be made next December will be to ascertain the characteristics of the solar corona. Observers will certainly be able to work much more effectually if they know beforehand the general nature of the phenomenon, for they will thus be guided not only in the selection of modes of observations, but also by knowing what points it is most important they should attend to.

I think it so essential to avoid raising unnecessary doubts, that I would not venture to express the opinion that the corona is wholly a solar appendage if I had not given the matter very careful consideration, and found the evidence overwhelmingly strong in favour of this view.

It is hardly necessary to discuss the theory that the corona is due to the diffraction of solar rays which pass near the Moon's edge, because that theory has been thoroughly disposed of by Brewster's arguments. Nor need we consider La Hire's theory that the phenomenon is due to the reflection of the solar rays from the irregularities of the Moon's surface, as it is obviously inconsistent with the observed peculiarities of the corona.

But a theory has recently been put forward that the corona is simply due to the glare of the terrestrial atmosphere, and this theory has been adopted by astronomers of standing. I hold it to be important, therefore, that this theory should be subject to careful scrutiny, as undoubtedly, if it be erroneous, much mischief may be done to the cause of scientific progress by its promulgation at the present conjuncture.

The first and most obvious evidence against this theory is the fact that the Moon is projected as a dark disk on the bright background (so to speak) of the corona. The theory requires that the corona should, in fact, not be a background, but a foreground; and one might naturally inquire how the Moon which is beyond the Earth's atmosphere should come to be apparently projected upon the supposed glare of that atmosphere.

But though this circumstance is in itself decisive of the matter at issue, let us turn to less obvious considerations. As a matter of fact, we know that light reaches the eye along lines tend ng from the neighbourhood of the eclipsed Sun. Let us inquire whether in those directions there is illuminated air; if not, optical considerations will force us to regard the source of light as beyond the air.

The eclipse of next December is not a favourable one for my argument; but it will be more interesting, and perhaps more useful, to consider it than any other.

In fig. 1, let A represent the position of an observer on the line of central eclipse, somewhere in the south of Spain. At such a station the eclipsed Sun will be almost 30 degrees above the horizon; and I find from a valuable paper which Mr. Hind has been good enough to forward to me, that the shadow-cone will

be about 50 miles across, where it reaches the Earth. Obviously, then, the shadow on the Earth will be an ellipse whose major axis will be about 100 miles, its minor about 50 miles in length. Let A S then be drawn inclined at an angle of about 30° to H A H', the horizon line at A, in a vertical plane through the Sun; and, having A *c*, A *c'* each to represent a space of 50 miles, let *c m* and *c' m'* be drawn, each inclined about 16′ to A *c*,

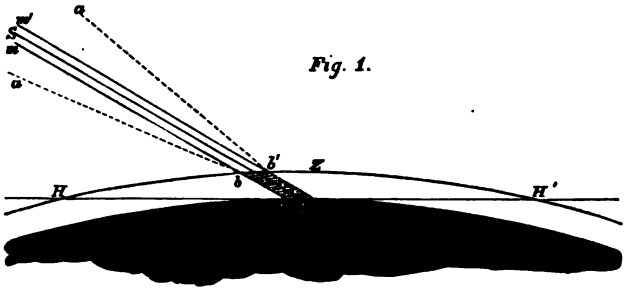


Fig. 1.

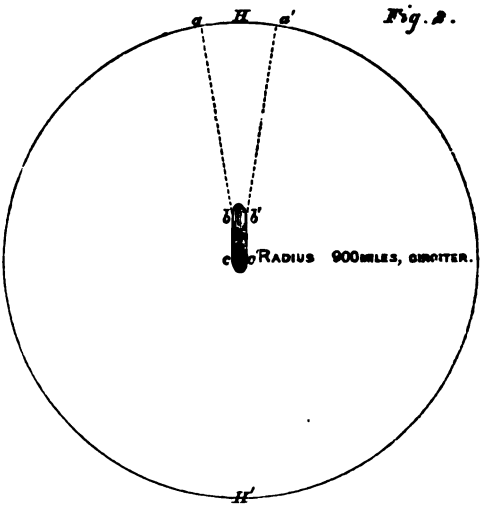


Fig. 2.

so that while A *c* is directed towards the Sun, *c' m'* and *c m* would be directed towards the highest and lowest points of the Moon's limb. Then *m' c' m c* is a vertical section of the Moon's shadow.

Now we do not know the height of the terrestrial atmosphere, but we may confidently believe that no air above the height of 100 miles can reflect any appreciable amount of solar light to

us.* Let us therefore take A Z to represent 100 miles; then H Z H′ will represent limits of the light-reflecting air, where H H′ is about 18 times as great as A Z. The portion of the atmosphere above the horizon-plane of the observer will therefore be of the figure produced by the revolution of H Z H′, about the vertical axis A Z. It will be, in fact, a plano-convex lens.

Let *c m* and *c′ m′* meet H Z H′ in *b* and *b′*; then the portion *b c c′ b′* will be in the Moon's shadow. (The effects of refraction are obviously insignificant.) The only light which can reach this part of the atmosphere is that from the chromosphere (to use a convenient but unsatisfactory name) and the coloured prominences, or from the earth and surrounding illuminated air. Towards *b* and *b′* the observer will recognise the first faint traces of directly illuminated atmosphere, and the light will gradually increase above *b′* and below *b* (more rapidly in the latter case than in the former). By a careful construction (a method quite exact enough for such an inquiry as the present), I make the the angle S A *a* about 6°, and the angle S A *a′* about 9°.

This, however, refers to only one section of the shadow-cone. To determine (roughly) the extent of illuminated atmosphere in a horizontal direction, we have only to consider the air-lens H Z H′ as supposed to be viewed from above. In fig. 2, *c c′* represents the actual shadow on the earth; *b b′* the intersection of the shadow-cone with the limits of our hypothetical envelope 100 miles high. Thus *b b′ c′c*, fig. 2, represents simply a vertical view of the portion *b c c′ b′* of the shadow-cone in fig. 1. Lines *b a*, *b′ a′*, drawn from the centre of the ellipse *c c′*, touching the ellipse *b b′*, give approximately the angular width of that part of the heavens within which no atmosphere directly illuminated by the Sun can be visible. I find from a careful construction that *a b* and *a′ b′* would include an angle of about 14½°.

Thus we obtain a nearly circular region (in which the Sun is eccentrically situated), having a horizontal diameter of about 14½°, and a vertical one of about 15°, within which there is not any light whatever from directly illuminated air. The Sun would be about 6° from the lowest point of this dark region.

With regard to the light from the prominences and the chromosphere, upon the air within this region, we know that it cannot suffice to light up the air with any strong, if even with

* Bravais, from a discussion of Lambert's observations of the crepuscular curve, deduced a height of nearly 100 miles. His own observations, made from the summit of the Faulhorn, gave a height of about 66 miles. Neither estimate refers to the actual limits of the atmosphere however. Dr. Balfour Stewart considers that perhaps the best means of judging on this point would be by observations made on the aurora. From such observations made in 1819, Dalton estimated the extreme height of the auroral light at 102 miles; Sir John Herschel estimated the height of an auroral arch seen on March 9, 1861, at 83 miles (undoubtedly the aurora is often seen much lower). The limits of air capable of reflecting light, must certainly lie much below the actual limits of the terrestrial atmosphere.

any appreciable glow; because we know how small a relation ordinary atmospheric glare bears to direct solar light, and the glare due to the chromosphere and prominences would bear a similar relation to the direct light from those sources. But further, whatever light came in this way, would obviously illumine the outer parts of the shadow-frustum *b b′ c′ c* more strongly than the parts near the axial line A S. Hence a faint diffused light diminishing *towards* the neighbourhood of the Moon should result.

As regards the illumination of our shadow-frustum by light derived from the neighbouring illuminated atmosphere and from the Earth, it is only necessary to remark that even when there is no eclipse the light thus falling on such a region as *b b′ c′ c* would be small; but that while a total eclipse is in progress all the parts near the shadow-cone are in nearly total eclipse, and not any part of the whole region H Z H′ is illuminated by so much as half the solar disk. Further, the light derived from this source, like that derived from the prominences and chromosphere, should diminish towards the neighbourhood of the Moon's disk, instead of increasing as the coronal light does. Also, the light from all these sources should extend over the Moon's disk, since it would illuminate the air between the observer and the Moon's body.

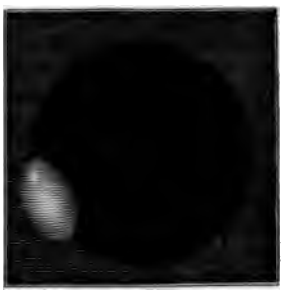
It follows, then, that so far from giving an account of the corona, atmospheric glare gives us a dark region round the eclipsed Sun, and a gradual increase of light with distance from him.

Within this dark space the disk of the Moon, illuminated by the Earth with about thirteen times as much light as the new Moon sends to us, ought to be conspicuous by its relative brightness.

Now, though the reasoning here deals with relations so simple that a mistake can hardly arise, yet there are certain tests to which these conclusions may be submitted before we proceed.

It is clear from fig. 1, that *before* the limits of the total shadow reached A there *should* be atmospheric glare towards the Sun, and further that this glare should at first wholly cover the Moon, and rapidly sweeping across her disk, just before totality, should pass away from her neighbourhood with undiminished velocity. It would be difficult to detect such a phenomenon by ordinary observation; though, as I shall presently show, not impracticable. But supposing a photograph could be taken an instant before totality, we might catch the glare while in the act of crossing the Moon's disk. Now this could only be managed by a miracle of dexterity; but by a miracle of good fortune, it *has* been managed already. The first photograph of Lieut.-Col. Tennant's admirable series was taken an instant before totality commenced; and *there we have the glare just about to leave the Moon's disk*,

Fig. 3.



but still trenching most obviously upon it. Fig. 3 represents the feature here dealt with. The light here is true atmospheric glare, and we see that, as might have been thought obvious, it is not limited by the Moon's disk which lies so far beyond the limits of the air. Then also we notice another important point. The edge of the glare is obviously travelling much faster than the Moon; for while the Moon proceeds to obliterate the last remaining point of the Sun's disk, the glare traverses the much wider distances separating its inner edge from the Moon's limb. Clearly this velocity would carry the glare clean away from the Moon, as the above reasoning shows should be the case

Again, it will be obvious, from a study of figs. 1 and 2, that during an annular eclipse, at the moment when the shadow-cone is pointing directly, or almost directly, towards the observer, the centre of the Moon's disk ought to be much darker than the edge. Now in the 10th volume of our *Memoirs*, Mr. Baily states that, while observing the eclipse of 1836, he noticed, on looking at the Moon through a telescope during the annularity, that "the circumference was tinged with a reddish purple colour which extended over the whole disk, but increased in density of colour according to the proximity to the centre, so as to be in that part nearly black." It is obvious that this appearance could last but a few seconds, since the moment the axis of the shadow-cone was turned appreciably away from the observer (and the vertex of the cone travels fully 20 miles per minute), he would be looking through the cone's *sides*. The following passage from Klein's *Sonnensystem* describes the whole phenomenon precisely in accordance with this view: "Bei der ringförmigen Finsterniss, am 30 October, 1864, sah Mouchez zu San Catharina in Brasilien, im Augenblicke als die Scheiben von Sonne und Mond concentrisch waren, das Centrum des Mondes völlig dunkel, aber von hieraus gegen dem Rand nahm die Helligkeit regelmässig zu und letzterer erschien heller, oder doch wenigstens ebenso hell, als das aschgraue Licht der Mondsichel, kurze Zeit vor oder nach dem Neumonde. Die ganze Erscheinung verschwand und die Mondscheibe war gleichförmig dunkel, als der leuchtende Ring gerissen und die Mitte der Finsterniss vorüber war."

Taking the corona to be a solar appendage, it is clear that even in total eclipses a somewhat similar appearance might be looked for, the outer parts of the Moon's disk during central totality seeming brighter than the centre, because the atmosphere between us and those parts would be more fully lighted up by the corona. I find, accordingly, that M. Tissel, observing the

total eclipse of 1733 at Skepshat, in Sweden, saw the Moon's surface brighter at the margin, and black towards the middle. We see from this most clearly that the atmospheric glare in this region is very much fainter than the corona, for except on a very close examination the Moon's disk, though the glare appears over a part of it during the totality, seems absolutely black, and is so rendered in photographs.

But further, if the views expressed above are correct, it ought to be possible under favourable circumstances to see the Moon's face by reflected Earth-light. I find that Bigerus Vassenius, during the remarkable total eclipse of 1733, using a telescope of 21 feet focal length, perceived the principal spots on the Moon during the total obscuration (*Phil. Trans.* 1733, p. 135). Ferrer also saw the spots on the Moon's surface very plainly during the total solar eclipse of 1806.

Yet again, if the apparent blackness of the Moon's disk results from the fact that the coronal light is beyond the Moon, and so forms the background on which she is projected,* two phenomena might be expected to be visible under favourable circumstances. First, the entire outline of the Moon's disk ought to be visible in partial eclipses, or before and after totality; and secondly, the corona ought to be visible at such times, and also, during annular eclipses. I find that the former phenomenon which corresponds in reality to the visibility of the corona (since were there no corona the Moon's limb could not appear dark where it crossed the Sun), has been frequently noticed; it has, in fact, been as often recognised as looked for. The visibility of the corona, when the Sun is not totally eclipsed, has also been so frequently recognised that it is hardly worth while to mention instances in point.† But I may quote, as very remarkable, the the fact that in 1860 Father Secchi saw the corona *for forty seconds after totality was past.* Another remarkable instance is that recorded by M. Edstrom, in the case of the eclipse of 1733, when the unequal radiations of the corona were observed to remain unchanged in position, as they gradually faded out of view with the increasing solar light.

It is further obvious, that if the corona be a solar appendage, one could expect it to appear concentric with the Moon only at

* The fact that the disk of *Venus* appears blacker than the surrounding sky when she is in superior conjunction, can only be explained by supposing there is some light beyond *Venus*. What can that light be but a solar appendage?

† Arago has founded on the visibility of the corona while a portion of the Sun is yet uneclipsed, a calculation of the ratio which the coronal light exceeds that of the atmospheric glare *then* undoubtedly present. That the corona is brighter than the atmospheric glare caused by a portion of the direct solar light undoubtedly follows from the visibility of the corona under such circumstances; but Arago's mode of treating the problem is not exact. He makes the atmospheric glare proportional to the portion of the solar disk visible at the moment. In reality, this proportion does not hold, for the upper regions of the air are illuminated by much more of the Sun's disk at such a time. In fact, the problem is one of much greater complexity than Arago seems to have imagined.

the moment of central eclipse. Now I find numerous instances in which it has been stated that quite obviously the brightest part of the corona was first on the side the Moon had just covered before totality, and lastly on the side she was just about to leave uncovered. I also find several statements (one or two very positive) that the corona was centrally disposed round the *Moon* throughout the totality. I would remark on this, that observations of the former kind, besides being more numerous, are severally more effective than observations of the latter kind. For the former refer to the recognition of a phenomenon and afford positive evidence; the latter merely assert the non-recognition of the phenomenon, and supply therefore only negative evidence. The former describe a peculiarity which attracted the notice of observers; the latter may be taken quite as well to indicate a want of skill in observation as the non-appearance of the particular phenomenon in question. All the positive evidence is therefore here also in favour of the view that the corona is a solar appendage.*

It remains that I should touch on other evidence we have of the existence of a solar appendage adequate to produce the observed appearances.

And first let us consider the zodiacal light. We know that even in our latitudes this phenomenon often exhibits a remarkable degree of luminosity towards the horizon and near the core of the gleam (so to speak). But in tropical countries the brightness of the zodiacal light is much more striking, and is seen to grow visibly greater in the Sun's neighbourhood. At heights of from 8000 to 12,000 feet in tropical climates, says Humboldt, the zodiacal light is seen of a brightness exceeding that of the Milky Way between *Aquila* and *Cygnus*. And obviously if we could trace the zodiacal light up to the solar limb, we should see it shining with a glory far exceeding that which it shows even in tropical countries. For we know that the brightest part there seen belongs still but to the outskirts of the object.†

* The centricity of the corona concerns, however, the question whether the corona is a solar or a lunar appendage; since the atmospheric glare should shift even much more obviously with respect to the Moon than a solar appendage would seem to do. No one now holds that the corona is a lunar appendage.

† I ought, perhaps, to show reason for regarding the zodiacal light as a solar appendage, notwithstanding Dr. Balfour Stewart's recent suggestion that it may be a terrestrial phenomenon. But in reality there can be no doubt whatever that the zodiacal light cannot be a phenomenon associated in any way with our atmosphere. Doubtless Dr. Stewart, whose mathematical attainments are well known, must have directed his attention too exclusively to the physical requirements of his theory, or he would not have overlooked obvious mathematical objections against it. The portion of the return-trade region above the horizon of any place is clearly a lamina shaped like a watch glass (slightly convex, see fig. 1), and the whole of this should be illuminated by electrical discharges excited in the way he suggests. We may, in fact, see in this an explanation of the familiar phosphorescence seen sometimes to cover the whole heavens (which gives the same spectrum as the aurora and zodiacal light), and even though at times, or even commonly, only a portion of this lamina should

Hence we should expect to find precisely such a glow of light round the Sun in total solar eclipses as we actually do see.

Again, from what we now know respecting meteors, we may derive abundant evidence in favour of the view that countless myriads of these bodies must always lie in the Sun's neighbourhood. For though, while the meteoric orbits were supposed circular, there was nothing very surprising in the fact that the Earth encountered so many as fifty-six meteor systems (because the zone of such systems seemed to lie close by the Earth's orbit), yet now we know how eccentric the meteoric orbits really are, we recognise the fact that antecedent probabilities would be wholly against the Earth's encountering even one such system, were there not many millions of them. And since she encounters fifty-six at least, we conclude that there must be millions on millions of such systems having their perihelion within the Earth's orbit. These uncounted systems ought to become visible during a total eclipse, since their dispersed members would lie in all directions round the Sun. Those meteoric flights also which were near him (and many must pass very near to him) would shine with a light whose brilliancy would go far to make up for the extreme relative minuteness of the individual meteors. Since near the Sun's disk the line of sight would be directed through a range of many millions of miles over which such meteors must be freely distributed, while along some 200 millions of miles in this direction meteors must be scattered, though more or less sparsely, one can recognise the reason of the brightness of the corona near the chromosphere.

Further, we know from the researches of Leverrier that there must exist continually in the Sun's neighbourhood a quantity of matter sufficiently important to affect the motion of the perihelion of *Mercury*. A few relatively considerable planets (as large say as the asteroids) might effect the observed changes; but far more

be so illuminated, no reason can be shown why that portion should always be an inclined tongue-shaped slip, as it should be to account for the zodiacal light in our latitudes. It is hardly necessary, however, to point out to the astronomical reader that a light which exhibits no parallactic displacement, which varies in position for different latitudes, according to the laws affecting the celestial bodies, which rises and sets according to the same laws, and which lastly effects the neighbourhood of the elliptic, cannot by any possibility belong to the Earth's atmosphere. The zodiacal light *might* be explained as due to a ring of matter surrounding the Earth, at a distance nearly equalling the Moon's, and travelling (as such a ring would) nearly in the plane of the ecliptic. Such an explanation was indeed put forward in 1856 by Prof. Heis. But the phenomena of the zodiacal light are much better explained by the theory that it is due to a solar appendage, even if we admit that the light sometimes extends from the eastern to the western horizon. But while Heis' theory, with overwhelming probabilities against it, has some points in its favour, the theory that the zodiacal light is an atmospheric phenomenon, is absolutely untenable. If anything would render the theory more strikingly opposed to observation than it is, it would be those occasional peculiarities of the zodiacal light which have been thought by some to favour the theory. These peculiarities simply add new difficulties to others already overwhelming.

probably a multitude of minute bodies may be held to be in question. Now the constant presence of meteors in the Sun's neighbourhood would produce the observed results, even though the individual meteors might remain but a brief time in the Sun's neighbourhood, to pass away presently on orbits whose aphelia might lie far beyond the orbits of the major planets.

Further, Mr. Baxendell has shown that certain peculiarities of magnetic and thermal change seem to point very decisively to the existence of a solar appendage holding the position which the corona, regarded as solar, seems to occupy. I have recently had the pleasure of discussing with him many of the relations considered above, and I find that there is nothing in his valuable meteorological researches which opposes itself to that particular view of the corona which I have advocated above, while his main result (which I hold to be of extreme importance) supplies an obvious argument in favour of that view.

Lastly, there are certain peculiarities in the aspect of the corona which seem only explicable on the theory above enunciated. Such are those radiations which are not at right angles to the Sun's limb; the phenomenon of loops of light in the corona with their concavity directed towards the Sun; the strange appearance resembling a hank of thread in disorder, seen by Arago in 1842; and other peculiarities too numerous to specify.

I know not of any phenomena which oppose themselves to the view here put forward, though I have carefully sought for such.

The spectroscopic analysis of the corona has not hitherto been altogether satisfactory, so that it may hardly be well to lay much stress upon it. It accords very satisfactorily, however, with the above theory. There would be a large quantity of reflected solar light in the corona, but there would also be much light from incandescent meteors, since those which came within a million or so of miles of the Sun would undoubtedly be raised to a white heat. Some of the meteors would, in all probability, be vapourised, and so bright lines might be seen in the coronal spectrum, as appears to have been the case during the total eclipse of last August. The observed association between meteors and comets suggests obvious considerations in explanation of the peculiarities said to characterise the spectrum of the corona. If the Great Comet of 1843 which passed within 65,000 miles of the Sun, has, like Tempel's comet, a train of meteoric bodies following in its track, these must be vapourised in the Sun's neighbourhood.

The contradictory evidence afforded by the polariscope is also obviously accounted for by the theory I have here advocated, even if it may not be said of itself, to force upon us the belief that the light of the corona is of that mixed kind which could scarcely result but in the way specified in that theory.

It would be desirable that measures should be adopted to insure the application of effective modes of observation during the very brief interval of total obscuration. I think this Society might with advantage appoint a committee to consider whether novel appliances and methods might not be employed to good purpose. The points I now proceed to touch on are so simple that some apology may, perhaps, be needed for bringing them under the notice of the Society; but if they should lead practised observers to make really important suggestions, my purpose will have been fulfilled.

In the first place, I would remark that observations specially directed to prove that the corona is a solar appendage would, in my opinion, be a complete waste of time and skill. It would be a misfortune — nay, it would even be in a sense discreditable — to astronomy, if the attention of observers should be directed to the solution of a question which has been practically solved during former eclipses. Unless the most obvious considerations of mathematics and optics are to be entirely neglected, the position of the corona as a solar appendage must be regarded as established, and all observations made with the object of confirming or disproving the matter, as simply futile.

But if we must travel over old paths, in order to establish that which is already evident, there are a few modes of observation which may be suggested as likely to give significant, however unnecessary, evidence.

If an observer were to confine his whole attention to the lunar disk during the eclipse, having a telescope with well-adjusted clock movement, and a field somewhat less than that of the full Moon, he would be able to recognise the following striking proofs of the real way in which the glare of the atmosphere varies during an eclipse. He would see, as the total phase approached, the atmospheric glare over the Moon's face gradually diminishing, and then what remained of actual glare from direct solar rays sweeping rapidly across the face of the Moon and leaving her disk relatively dark. But in a few moments the observer would be able (in favourable atmospheric circumstances) to recognise the spots on the lunar surface.

If an observer were to limit his attention to the Moon's disk during totality, keeping his eyes in darkness until the commencement of totality was announced by those around him, he would be certainly able to see the lunar spots, unless atmospheric conditions were very unfavourable indeed.

Attention might be directed to the shape and motions of the dark region of the sky surrounding the corona; and such observations would not be so complete a waste of time as those last considered, since it is evident that important information might be gathered from them respecting the height of the atmosphere. Such information would be in many respects more trustworthy than that which has been derived from the position, shape, and motions of the crepuscular curve.

But a mode of observation which I would advocate with great earnestness, is the simple application of telescopic power to determine, if possible, the structure of the corona. I have no doubt that this structure is continually changing; but most valuable information might be gained from a careful study of the position of the coronal beams at the time, and of those singularly complex hanks and streamers which have been already noticed by astronomers. The use of a telescope of low magnifying power, but first-rate definition, a comet eye-piece being employed, would be desirable in thus studying the corona. The telescope should be accurately driven by clock-work, and a dark iris-disk, if I may so describe an arrangement which would be the converse of an iris diaphragm, might be employed with advantage to hide the light of the prominences and chromosphere. If the field of view were several degrees in diameter, and the dark disk at the beginning of totality concealed a circular space extending a degree or so beyond the eclipsed Sun, the observer might first examine with great advantage the outer parts of the corona, and gradually extend his scrutiny to the very neighbourhood of the prominences. Supposing his eyes had been kept in darkness before totality began, he would be able to gain such an insight into the real structure of the corona as has never yet been obtained by astronomers.

As regards the spectroscopic and polariscopic analysis of the corona I shall say little. It would obviously be most desirable that Mr. Huggins, Mr. Lockyer, and those astronomers whose attention has been practically directed to researches of this sort, should give careful consideration to the question how the short interval of totality may best be employed, and that they should make their views public as early as possible. To one point, however, I shall venture to direct the attention of observers. It seems to me most important that every observer proposing to take part in applying such delicate light-tests to the corona, should prepare for the observations he is to make by keeping his eyes in darkness as nearly complete as possible for some time before totality commences; and further, where different parts of the corona are to be examined, the fainter parts should be first dealt with.

If the search for an intra-Mercurial planet is to be renewed with any chance of success, there can be little doubt that the telescopist must keep the corona, or at least its brightest portions, out of the field of view. A telescope specially constructed for the purpose, having a motion carrying the tube conically round a mean position might easily be devised; and with such an instrument one might conveniently sweep the Sun's neighbourhood all round the limits of the corona, for *Vulcan* and perhaps a train of attendant *Cyclopes*. But a telescope of low power, with a comet eye-piece, and a diaphragm concealing the brighter part of the corona, would probably be quite as effective.

For this class of observation, also, it would be very advan-

tageous that the eyes should be kept in darkness for some time before totality commenced.

Are observers to be found who, supposing the circumstances of the coming eclipse to be favourable, will be ready to forego the opportunity of witnessing one of the grandest of all natural phenomena, of watching the gathering shadows, of beholding the wonderful transformation of the face of nature, the weird and unearthly aspect of all things round them, and the strange beauty of the solar corona of glory, in order that they may devote all their observing energies during two short minutes to important but severally uninteresting, phenomena? We know that, so far as the period of totality is concerned, such a sacrifice has already been made by De La Rue and Tennant, by Secchi, Janssen, Lieut. Herschel, Young, and a number of other lovers of science; but no observer has yet foregone the whole spectacle of a total eclipse for the sake of the dull, dry details of scientific observation.

Ephemeris of the Satellites of Uranus. By A. Marth, Esq.

Angles of Position at 8^h Greenwich Mean Time.

1870.	Ariel.	Umbriel.	Titania.	Oberon.
	°	°	°	°
Feb. 18	152	70	317	161
19	3	346	272	137
20	217	258	223	109
21	79	172	185	77
22	303	85	150	46
23	157	357	108	19
24	8	273	60	357
25	223	182	17	334
26	87	100	342	309
27	309	7	305	281
28	162	288	257	248
Mar. 1	13	193	211	218
2	229	114	174	193
3	93	18	139	170
4	314	301	95	148
5	167	204	46	122
6	18	128	7	92
7	236	30	332	60
8	100	314	292	31
9	320	216	242	7
10	172	140	200	345
11	24	42	165	321

1870.	Ariel. °	Umbriel. °	Titania. °	Oberon. °
12	243	325	127	294
13	107	229	80	263
14	325	151	33	232
15	176	56	357	204
16	29	336	321	181
17	250	243	277	159
18	114	162	229	135
19	330	71	189	106
20	181	347	154	75
21	35	257	114	44
22	256	172	65	18
23	120	86	22	355
24	335	357	346	333
25	186	274	310	308
26	41	182	263	278
27	264	101	216	246
28	126	8	179	216
29	340	288	144	192
30	191	193	100	169
31	47	115	51	146
Apr. 1	271	19	11	120
2	132	302	336	90
3	345	205	297	58
4	196	128	249	29
5	53	31	204	5
6	278	314	169	343
7	137	217	132	320
8	350	141	86	293

The Apparent Distances vary between the Limits

Ariel	15″	and	12″
Umbriel	21		16
Titania	35		27
Oberon	46		36

The Zodiacal Light. By Capt. Noble.

On the evening of March 3rd, from $7^h\ 40^m$ to 8^h, L.M.T., s phenomenon was much brighter and more conspicuous than ıave ever seen it before, surpassing in vividness that part of ɜ Milky Way running through *Cepheus* and *Cygnus*, which ıs, of course, favourably situated for comparison. It is stated most of the popular books on Astronomy, that the axis of the

Zodiacal Light nearly coincides with the ecliptic; but, on the occasion to which I refer, it certainly trended considerably to the right of it (as viewed with the naked eye). The boundaries of the light were not well defined. It involved *α*, *β*, and *γ Arietis*, and extended a little way upwards towards *Andromeda*. The *Pleiades* were markedly to the left of it, and separated from it by an unmistakable gap of dark sky. Allowing for the difficulty of estimating accurately the axis of a figure, which requires averted vision to reduce to anything like shape; and speaking roughly, I should say that such axis was inclined some 20° to the ecliptic, according to the best estimation that I could form.

Forest Lodge, Maresfield, Sussex,
March 10th, 1870.

Occultation of a Star by the Moon. By Capt. Noble.

Thursday, February 10th, 1870, *m Tauri* disappeared instantaneously at the Moon's dark limb at $6^h\ 33^m\ 30^s{\cdot}2$, L.S.T.= $9^h\ 10^m\ 33^s{\cdot}1$, L.M.T. and reappeared at the bright limb somewhere about $7^h\ 12^m\ 45^s$, L.S.T.=$9^h\ 49^m\ 41^s{\cdot}5$, L.M.T. This was an unsatisfactory observation, as no determination was made of the errors of adjustment of the Transit when the time was taken; and, in addition, the reappearance was badly seen, the star being clear of the Moon's limb when first glimpsed. Power, 255, adjusted on the star.

Observations of Venus near her inferior Conjunction. By Capt. Noble.

The day on which the Planet was actually in inferior conjunction was densely cloudy here; but on the previous one, Tuesday, February 22nd, I observed her at $2^h\ 10^m$, L.M.T. when she was within $24^h\ 14^m$ of such conjunction. She presented the appearance of an exquisitely delicate thread of light, the line joining the cusps being a chord less than a diameter: in other words, the hair-like luminous line did not extend round a semicircle. A defect in the driving-clock of my Equatoreal precluded me from making any micrometrical measurements, however, and this must be my excuse for speaking thus vaguely. Constricting the field of view of a Huyghenian eye-piece magnifying 154 times by means of a card diaphragm pierced with a central needle line, I could see, plainly enough, the dark body of the planet. The sky was somewhat hazy, and I could not trace the dark limb *quite* round; but its difference of tint from that of the surrounding sky was evident the instant *Venus* was regarded. I employed powers of 74, 115, and 154. Vision was most satisfactory with the latter.

Note on further changes in the Coloured Belt of Jupiter.

By John Browning, Esq.

The coloured belt on Jupiter to which I have recently had the honour of calling the attention of the Society, has undergone many changes both in form and hue since I last described the appearance of the planet.

The ochreish-yellow colour is rather fainter, and of a duskier hue, and it is confined to the northern part of the equatorial belt, instead of covering the whole of it as was formerly the case.

It would, perhaps, be more correct to say that there are two belts near the equator, one to the north, of a faint dusky yellow, not dark, but very dim, and one to the south, which is pure white. This belt is by far the brightest portion of the planet's disk, and it is the only portion of the disk which is colourless.

The drawing I have the honour to exhibit represents pretty faithfully the appearance of the planet on January 31st, at 9.30, G.M.T.

March 10th, 9.30 P.M.—Observed *Jupiter* with reflector, 12-in. aperture, and power 140, achromatic eye-piece. The planet being very low, I could not use a higher power with advantage. The tawny yellow colour now again extends over the whole of the equatorial belt, which is broader than I have ever seen it before. This belt has a very dark band on the south, and a narrower dark band on the north; beyond each of these there is a very brilliant white belt. These two belts are the brightest portions of the planet's disk. As this striking outbreak of colour appears to be on the increase, it is very desirable that those observers who have a good western view should observe the planet, and record their observations at every possible opportunity, using the Astronomer Royal's new correcting eye-piece for the purpose.

Minor Planet (109) *Felicitas.*

Discovered 9th October, 1869, by Dr. C. H. F. Peters, at Clinton, N. Y.

The following Elements, calculated by him from observations up to 9th December, are given with an Ephemeris, *Ast. Nach.* No. 1788:—

Epoch, 1870, Jan. 0·0 Berlin M.T.

$M_0 = 357^\circ\ 33'\ 46''\cdot82$

$\pi = 55\ 53\ 6''\cdot07 + 50''\cdot24\ t$

$\Omega = 4\ 57\ 12''\cdot20 + 40''\cdot59\ t$

$i = 8\ 3\ 17''\cdot56 + 0''\cdot46\ t$

$\varphi = 17\ 27\ 5''\cdot47$

$\mu = 801''\cdot820$

$\log a = 0\cdot4306198$

Where t is reckoned in years from 1850·0.

ERRATA.

Page 43, line 27, *for* Fareham, *read* Farnham.

Page 134, "Dome for Sale," *for* 25 feet diameter, *read* 15 feet diameter.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *April* 8, 1870. No. 6.

WILLIAM LASSELL, Esq., President, in the Chair.

Lieut. Abney, Royal Engineers,
R. Abbay, Esq. Wadham College,
W. Thirlwall Bayne, Esq., 34 Jermyn Street,
C. J. Lambert, Esq., Pembroke College, Cambridge,
Dr. Riches, 1 Lansdowne Crescent, Bath, and
Geo. M. Seabroke, Esq., Rugby,

were balloted for and duly elected Fellows of the Society.

The following Circular has been, by direction of the Council, issued to the Fellows of the Society:—

Sir,—We are instructed to communicate to you the following resolution, which was passed at a Committee of the Council held yesterday, April 8th,

Resolved—"That the Fellows be informed that there is a possibility of the Government providing means of transit to and from stations on the Mediterranean for about sixty observers, who may be willing to take part in the Observation of the Total Eclipse of December 22, 1870; and that persons willing to undertake a portion of the Observation, on a plan to be arranged by the Council, be invited to send their names to the Secretaries, and also to state the branch of observation which they would be prepared, or prefer, to undertake, and the instruments they would be willing to contribute."

It is desirable that the names of those who are willing to take part in the observation of the Eclipse should be sent in, if possible, before the next Meeting of Council, May 13.

We have the honour to remain, Sir, yours faithfully,

WILLIAM HUGGINS,
E. J. STONE, } *Hon. Secs.*

Royal Astronomical Society, April 9, 1870.

On the Orbit of the Comet of 1683. By Mr. W. E. Plummer, of Mr. Bishop's Observatory, Twickenham. (Note by Mr. Hind.)

The comet of 1683 was first seen by Flamsteed on the 23rd of July, and was assiduously observed at Greenwich until the 5th of September. The original observations are printed in the first volume of the *Historia Cœlestis*. Hevelius observed the comet at Dantzic from the 30th of July to the 4th of September, and the particulars of his observations appear in his *Annus Climactericus*.

Halley calculated a parabolic orbit for this comet from Flamsteed's observations, which was published in his *Synopsis of Cometary Astronomy*, and is as follows :—

Perihelion Passage, 1683, July 13, at $3^h\ 0^m$, G.M.T.

Longitude of Perihelion	85° 29′ 30″
Longitude of Ascending Node ..	173 23 0
Inclination of the Orbit	83 11 0
Logarithm of Perihelion distance ..	9·748343

Motion Retrograde.

In the fifth volume of the *Astronomische Nachrichten* (No. 117) Clausen (the present director of the Observatory at Dorpat) has given elliptic elements for this comet, founded as he states upon a new reduction of Flamsteed's observations. These elements are as follow :—

Perihelion Passage, 1683, July 12·73236, M.T. at Paris.

Longitude of Perihelion	86° 31′ 15″	Mean Equinox, 1683·0.
Longitude of Ascending Node ..	173 17 48	
Inclination of the Orbit	83 47 46	
Logarithm of Perihelion distance ..	9·7430148	
Eccentricity	0·9832470	

Motion Retrograde.

The period of revolution in this ellipse is 189·8 years, which would bring the comet to perihelion again in 1873.

Mr. Bishop having, on my suggestion, requested Mr. Plummer, assistant in his observatory, to make a new and rigorous reduction of Flamsteed's observations, an accurate Ephemeris of the comet was first calculated for the whole interval between the extreme observations, the Sun's longitudes and the radii-vectores of the Earth, being deduced from the Solar Tables of Leverrier. The places of the comparison stars were carried back, from the Greenwich Catalogue for 1860, undoubtedly the highest existing

authority. The observed distances of the comet from stars were carefully corrected for refraction and parallax, a mean value of the refraction being employed, and the distances in the computation of parallax inferred from Clausen's ellipse. A comparison of the observations thus rigorously reduced with Clausen's orbit was then made, and it was at once evident that an orbit might be found to represent the observations much more satisfactorily than the ellipse in question. This Mr. Plummer proceeded to investigate, and the result is a parabolic orbit which accords as well with the whole course of observations as the limits of their errors will allow. The elements of this parabola are :—

Perihelion passage, 1683, July 13, 09068, Greenwich M.T.

Longitude of Perihelion	85° 35′ 59″·5	Mean Equinox, 1683·0.
Longitude of Ascending Node ..	173 24 39·7	
Inclination of the Orbit	83 13 14·7	
Logarithm of Perihelion distance ..	9·7478656	

Motion Retrograde.

The sums of the squares of the errors are :—

	R.A.	N.P.D.
In Clausen's ellipse ..	337278″	224845″
In Plummer's parabola ..	29089″	58394″

It would therefore appear that there is no reason to expect the return of the comet within the next few years, or to suppose that it is one of moderate period.

If I am not greatly mistaken, Clausen has merely reduced Flamsteed's observations on three days, and computed an orbit upon them by a general method, such as that given by Gauss in the *Theoria Motus*. The observations selected are almost indicated by the comparison with his elements.

I should add that the observations of Hevelius were likewise rigorously reduced, but being found greatly inferior to Flamsteed's in point of accuracy, it was considered that no advantage would be gained by employing them in the calculations for the orbit.

It is the intention of Mr. Bishop, to publish the details of Mr. Plummer's investigation in a separate form.

1870, *April* 7.

Studies on the frequency of Sun-spots, and on their connexion with the Magnetic Declination-variation. By Prof. Rudolf Wolf. (Translation.)

Denoting by r the Relative-number for the Sun-spot frequency introduced by me in the year 1850, by f the number of days with-

out spots, and by b the number of days of observation, I obtain for 1864–69 the following table:—

	1864.		1865.		1866.	
	r	$f:b$	r	$f:b$	r	$f:b$
January	57·5	0 : 31	48·3	0 : 30	33·3	0 : 31
February	47·2	0 : 29	44·8	0 : 27	39·4	1 : 28
March	67·3	0 : 31	40·7	2 : 28	27·2	0 : 29
April	30·0	2 : 29	32·5	1 : 30	18·9	2 : 30
May	40·9	0 : 31	37·5	2 : 31	15·0	6 : 31
June	58·3	0 : 30	36·3	2 : 30	18·3	3 : 30
July	57·2	0 : 31	29·7	2 : 31	10·2	9 : 31
August	57·9	2 : 31	40·3	0 : 31	14·0	5 : 31
September	30·5	1 : 30	22·9	7 : 30	8·0	13 : 30
October	35·5	0 : 31	18·5	10 : 31	14·6	5 : 31
November	59·1	0 : 28	24·7	4 : 27	9·3	16 : 30
December	24·1	1 : 24	13·3	9 : 28	1·6	26 : 30
Year	47·1	6 : 356	32·5	39 : 354	17·5	86 : 362

	1867.		1868.		1869.	
	r	$f:b$	r	$f:b$	r	$f:b$
January	0·0	29 : 29	12·2	13 : 25	72·4	0 : 27
February	0·8	26 : 28	16·4	5 : 28	72·4	1 : 24
March	10·8	11 : 31	28·7	3 : 30	65·3	0 : 28
April	5·8	20 : 30	39·4	0 : 30	46·5	1 : 30
May	3·3	24 : 31	30·3	3 : 31	115·8	0 : 30
June	1·6	26 : 30	34·7	2 : 30	120·4	0 : 30
July	5·3	18 : 31	32·2	10 : 31	65·1	1 : 31
August	5·9	19 : 31	38·6	0 : 31	93·2	0 : 30
September	10·6	15 : 30	52·6	1 : 30	88·5	0 : 30
October	14·2	13 : 31	60·5	0 : 29	62·4	0 : 30
November	10·3	9 : 30	67·9	0 : 21	85·7	0 : 24
December	27·5	6 : 24	68·4	0 : 29	122·1	0 : 25
Year	8·0	216 : 356	40·2	32 : 345	84·1	3 : 339

One recognises herein, at the first glance, the minimum of 1867 according with my Sun-spot period of $11\frac{1}{9}$ years, and from that date the rapidly increasing Sun-spot frequency. By the empirical formula which, since 1859, I proposed for various points in order from r to deduce the declination-variation b, I give here, only by way of example, the calculated formula 18[illegible]-61 for Christiania,—

$$v = 0'{\cdot}0413$$

This gives the following table, [illegible] the [illegible] calculated by the formula, v' the v[illegible] from t[illegible]

observations, communicated to me by Messrs. Mohn and Fearnley.

	1864.	1865.	1866.	1867.	1868.	1869.
ϑ	6′·87	6′·26	5′·64	5′·25	6′·58	8′·39
ϑ'	6·00	5·72	5·70	5·69	6·65	7·82

It is hardly necessary to remark that the constants of the formulæ may, by means of the whole series of the Christiania Observations, easily be altered so as to obtain a yet better agreement. I prefer, however, for the present not to make this change.

Zurich, March 28, 1870.

Observations of Lunar Eclipse, Jan. 17, 1870.

By J. Tebbutt, jun.

The lunar eclipse of the 17th instant was remarkably well seen here. The Moon was overspread with very thin filmy cloud till about $11^h\ 43^m$, but the diminution of her brilliancy from that cause was very slight. She remained unclouded during the rest of the phenomenon. No decided defalcation of light was noticed on the eastern limb till $10^h\ 41^m$, but at $10^h\ 52^m$ the effects of the penumbra were very marked. The following are the local mean times of the different phases as near as they could be observed, it being a most difficult matter to fix the precise instants of the contacts owing to the ill-defined character of the shadow:—

	d	h	m	s
First contact with the shadow	17	11	1	19
Beginning of the total phase	..	12	0	29
End of the total phase	..	13	38	53
Last contact with the shadow	..	14	38	58

At $11^h\ 29^m$ the shadow assumed a light copper tint, except at its periphery, where it was of a very dark green. The copper tint, as seen in the telescope, appeared to extend even to the filmy cloud which lay along the Moon's eastern limb. At $11^h\ 43^m$, when the Moon shone unclouded, the details on the obscured portion of the lunar surface began to be perceptible in the telescope. These became gradually more distinct, and it was soon observed that the dark body of the Moon was surrounded by numerous telescopic stars, and that many occultations would occur during the total phase. Several of these phenomena were noted with tolerable accuracy; some of the stars, however, were faint for accurate observation. The following occultations recorded:—

Star.	Mag.	Phase.	Mean Time. h	m	s	Remarks.
a	7	Disappearance	11	59	24·7	Disappearance sudden.
b	8½	,,	12	49·4		(Approx.)
c	8	,,	13	2	19·3	Disappearances near upper limb: a little uncertain.
d	8	,,	13	5	0·2	
e	8	Reappearance	13	6	18·7	
a	7	,,	13	25	4·2	(Approx.)
f	7	Disappearance	13	36	24·2	Uncertain to a second, owing to increasing brightness of limb.

The noted time of the reappearance of the star *a*, owing to a temporary removal of the eye from the telescope, was probably two or three seconds late. The Moon's disk was of a copper hue throughout the total phase, and continued distinctly visible both to the naked eye and in the telescope. The southern limb was remarkably bright at the middle of the eclipse. The meridian transit of the first limb was pretty well observed, but the second limb was too faint. The copper and dark green tints were again observed after the total phase, that portion of the obscured surface next to the centre of the shadow being copper-tinted, and the outline of the shadow being very dark green. The telescopic observations during the eclipse were all made with my refractor of 3¼ inches aperture, and 48 inches focal length, furnished with a magnifying power of about 30.

Windsor, New South Wales, Jan. 26, 1870.

A few further Notes on the Floor of Plato.

By W. R. Birt, Esq.

In my communication on the floor of *Plato*, which was read at the Meeting of the Society in November last, I solicited attention to a feature presented by the spots, and which I ventured to term "degree of visibility." The paper had reference to twenty-five spots, the degree of visibility of sixteen being given. The entire number of observations to Sept. 27, 1869, was 238. Since that date the observations have been continued, and greater attention has been given to the subject, and as twelve months' observations are now completed, including a luni-solar year, the following results may not be uninteresting.

The number of spots which have been seen on the floor of *Plato* up to the present time is thirty-five, eight of which have been detected since Sept. 27, 1869. The number of observations since that date amounts to 531, being more than double the number (240) during the first six lunations of the year 1869, April to 1870 March inclusive. In the following table the "degree of visibility" of each spot is given for the first six lunations, the last six lunations, the increase or decrease of visibility of

those spots which are comparable, and the "degree of visibility" of each spot for the year, the number of observations being 771.

No.	April to Sept. Obs.	Vis.	Oct. to March. Obs.	Vis.	Diff.	Year. Obs.	Vis.
0	..	..	5	·070	..	5	·048
1	33	1·000	71	1·000	..	104	1·000
2	1	·030	6	·084	+·054	7	·067
3	24	·727	68	·958	+·231	92	·885
4	28	·848	62	·873	+·025	90	·865
5	20	·606	30	·423	−·183	50	·481
6	8	·242	24	·338	+·096	32	·308
7	8	·242	8	·113	−·129	16	·154
8	..	..	3	·042	..	3	·029
9	5	·151	20	·282	+·131	25	·240
10	6	·182	5	·070	−·112	11	·106
11	1	·030	14	·197	+·167	15	·144
12	3	·091	1	·014	−·077	4	·038
13	16	·485	12	·169	−·316	28	·269
14	20	·606	30	·423	−·183	50	·481
15	1	·030	2	·028	−·002	3	·029
16	12	·364	20	·282	−·082	32	·308
17	22	·666	62	·873	+·207	84	·808
18	1	·030	6	·084	+·054	7	·067
19	17	·515	10	·141	−·374	27	·260
20	3	·091	5	·070	−·021	8	·077
21	..	..	4	·056	..	4	·038
22	10	·303	15	·211	−·092	25	·240
23	1	·030	3	·042	+·012	4	·038
24	..	..	5	·070	..	5	·045
25	..	..	9	·127	..	9	·085
26	..	..	1	·014	..	1	·010
27	..	..	2	·028	..	2	·019
28	..	..	1	·014	..	1	·010
29	..	..	6	·084	..	6	·058
30	..	..	8	·113	..	8	·077
31	..	..	1	·014	..	1	·010
32	..	..	6	·084	..	6	·058
33	..	..	1	·014	..	1	·010
34	..	..	5	·070	..	5	·048

On running the eye along the column of differences it will be seen that the number of spots in which an increase has taken place during the last six lunations is nearly equal to that in which a decrease has occurred, viz. ten of the former and eleven of the latter. Spot No. 3, a craterlet, has manifested the greatest increase, while spot No. 19 has exhibited the largest decrease.

The extent of variation of the separate spots is very irregular, and does not appear to indicate the operation of any general law. In one or two instances only have neighbouring spots been similarly affected; thus spots Nos. 5 and 14 in the S.W. quadrant of *Plato*, exhibit the same *decrease* of visibility, and the way in which they have varied from lunation to lunation is somewhat similar, and unlike the manner in which most of the other spots have varied. Spots No. 2 and 18 exhibit the same *increase* of visibility. The great increase of white spots in every part of the Moon's disk, about the time of full, dependent upon the value of $\mathfrak{C} - \odot$ would, lunation after lunation, contribute to a steady value of the degree of visibility rather than the irregularity which is indicated by the observations if the *same* spots had been seen month after month. Although the observations amount to 771, and as many as twenty-two spots have been observed on one evening, the average number visible at any given time, as deduced from the 108 series of observations, is seven only, a number which is constant. Upon examining those series in which a smaller number than seven has been recorded, it is found that, besides the spots most commonly seen, viz. Nos. 1, 3, 4, and 17, the remaining two have not been the same. The additional spots seen on these occasions have been very various, several of them having low degrees of visibility, and some of these, which it might be expected could be seen only in the finest weather, have been observed in ordinary states of the Earth's atmosphere.

The observations of the twelve lunations ending in March, 1870, extend considerably the basis on which to found an intelligible explanation of the phenomena, it is, nevertheless, much too narrow to hazard more than conjecture. Another year's observations will doubtless throw further light on the subject.

On the Graphical Construction of the Umbral or Penumbral Curve at any instant during a Solar Eclipse. By Prof. Cayley.

The curve in question, say the penumbral curve, is the intersection of a sphere by a right cone,—I wish to show that the stereographic projection of this curve may be constructed as the envelope of a variable circle, having its centre on a given conic, and cutting at right angles a fixed circle; this fixed circle being in fact the projection of the circle which is the section of the sphere by the plane through the centre and the axis of the cone, or say by the axial plane. The construction thus arrived at is Mr. Casey's construction for a bicircular quartic; and it would not be difficult to show that the stereographic projection of the penumbral curve is in fact a bicircular quartic.

The construction depends on the remark that a right cone is the envelope of a variable sphere having its centre on a given line, and its radius proportional to the distance of the centre

from a given point on this line; and on the following theorem of plane geometry:

Imagine a fixed circle, and a variable circle having its centre on a given line, and its radius proportional to the distance of the centre from a given point on the line (or, what is the same thing, the variable circle always touches a given line); then the locus of the pole in regard to the fixed circle, of the common chord of the two circles (or, what is the same thing, the locus of the centre of a new variable circle which cuts the fixed circle at right angles in the points where it is met by the first-mentioned variable circle) is a conic.

To fix the ideas, say that P is the centre of the first variable circle; A B its common chord with the fixed circle; Q the centre of the circle which cuts the fixed circle at right angles in the points A and B; then the locus of Q is a conic.

To prove this, take $x^2 + y^2 = 1$ for the equation of the fixed circle $(x - \alpha)^2 + (y - \beta)^2 = \gamma^2$ for that of the variable circle; the foregoing law of variation being in fact such that α, β, γ, are linear functions of a variable parameter θ; the equation of the common chord A B is $-2\alpha x - 2\beta y + 1 + \alpha^2 + \beta^2 - \gamma^2 = 0$; viz., this equation contains θ quadratically; hence the envelope of the common chord is a conic; and thence (reciprocating in regard to the fixed circle) the locus of the pole of A B, that is, of the point Q, is also a conic.

Consider now a solid figure in which the circles are replaced by spheres; viz. we have a fixed sphere, and a variable sphere having its centre on a given line and its radius proportional to the distance of the centre from a given point on the line. The envelope of the variable sphere is a right cone; the intersection of the cone with the fixed sphere is the envelope of the small circle of the sphere, say the circle A B, which is the intersection of the fixed sphere by the variable sphere. This circle A B is also the intersection of the fixed sphere by a sphere, centre Q, which cuts the fixed sphere at right angles; and by what precedes the locus of Q is a conic. Hence the penumbral curve is given as the envelope of the circle A B which is the intersection of the fixed sphere by a sphere which has its centre Q on a conic, and which cuts the fixed sphere at right angles. It is obvious that the circle A B always cuts at right angles the great circle which is the section of the fixed sphere by the axial plane, or say the axial circle. Project the whole figure stereographically; the projection of the circle A B is a variable circle which cuts at right angles the circle which is the projection of the axial circle, and which has for its centre the point Q′ which is the projection of Q. But the locus of Q being a conic, the locus of its projection Q′ is also a conic; and we have thus the projection of the penumbral curve as the envelope of a variable circle which has its centre on a conic, and which cuts at right angles a fixed circle.

We may in the axial plane construct five points of the conic which is the locus of Q, by means of any five assumed positions

of the variable circle, and somewhat simplify the construction by a proper choice of the five positions of the variable circle. This is not a convenient construction, and even if it were accomplished we should still have to construct the projection of the conic so obtained, in order to find, in the figure of the stereographic projection, the conic which is the locus of Q′. I do not at present perceive any direct construction for the last-mentioned conic; but assuming that a tolerably simple construction can be obtained, the construction of the projection of the penumbral curve as the envelope of the variable circle is as easy and rapid as possible. Probably the easiest course would be (without using the conic at all) to calculate numerically, for a given position of the variable sphere, the terrestrial latitude and longitude of the two points of intersection of the variable sphere by the axial circle; laying these down on the projection, we have then a position of the variable circle; and a small number of properly selected positions would give the penumbral curve with tolerable accuracy.

I have throughout spoken of the penumbral curve, as it is in regard hereto that a graphical construction is most needed; but the theory is applicable, without any alteration, to the umbral curve.

On the Geometrical Theory of Solar Eclipses.

By Prof. Cayley.

The fundamental equation in a solar eclipse is, I think, most readily established as follows:—

Take the centre of the Earth for origin, and consider a set of axes fixed in the Earth and moveable with it; viz., the axis of z directed towards the North Pole; those of x, y, in the plane of the Equator; the axis of x directed towards the point longitude 0°; that of y towards the point longitude 90° W. of Greenwich. Take a, b, c, for the co-ordinates of the Moon; k for its radius (assuming it to be spherical); a', b', c', for the co-ordinates of the Sun; k' for its radius (assuming it to be spherical); then, writing $\theta + \varphi = 1$, the equation

$$\{\theta(x-a) + \varphi(x-a')\}^2 + \{\theta(y-b) + \varphi(y-b')\}^2 + \{\theta(z-c) + \varphi(z-c')\}^2 = (\theta k \pm \varphi k')^2$$

is the equation of the surface of the Sun or Moon, according as $\theta, \varphi = 1, 0$ or $= 0, 1$: and for any values whatever of θ, φ, it is that of a variable sphere, such that the whole series of spheres have a common tangent cone. Writing the equation in the form

$$\begin{aligned} &\theta^2\{(x-a)^2 + (y-b)^2 + (z-c)^2 - k^2\} \\ &+ 2\theta\varphi\{(x-a)(x-a') + (y-b)(y-b') + (z-c)(z-c') \mp k k'\} \\ &+ \varphi^2\{(x-a')^2 + (y-b')^2 + (z-c')^2 - k'^2\} = 0, \end{aligned}$$

or, putting for shortness,

$$\begin{aligned}
\varrho &= a^2 + b^2 + c^2 - k^2 \\
\varrho' &= a'^2 + b'^2 + c'^2 - k'^2 \\
\sigma &= a a' + b b' + c c' \mp k k' \\
\mathrm{P} &= a x + b y + c z \\
\mathrm{P}' &= a' x + b y + c' z,
\end{aligned}$$

the equation is

$$\begin{aligned}
&\theta^2 (x^2 + y^2 + z^2 - 2\mathrm{P} + \varrho) \\
+\ 2\theta\varphi &(x^2 + y^2 + z^2 - \mathrm{P} - \mathrm{P}' + \sigma) \\
+ \ \varphi^2 &(x^2 + y^2 + z^2 - 2\mathrm{P}' + \varrho') = 0
\end{aligned}$$

and the equation of the envelope consequently is

$$(x^2 + y^2 + z^2 - 2\mathrm{P} + \varrho)(x^2 + y^2 + z^2 - 2\mathrm{P}' + \varrho') - (x^2 + y^2 + z^2 - \mathrm{P} - \mathrm{P}' + \sigma)^2 = 0$$

that is

$$(x^2 + y^2 + z^2)(\varrho + \varrho' - 2\sigma) - (\mathrm{P} - \mathrm{P}')^2 - 2(\varrho' - \sigma)\mathrm{P} - 2(\varrho - \sigma)\mathrm{P}' + \varrho\varrho' - \sigma^2 = 0$$

which is the equation of the cone in question.

Observe that one sphere of the series is a *point*, viz., taking first the upper signs, if we have $\theta k + \varphi k' = 0$, that is

$$\theta = \frac{k'}{k' - k}, \quad \varphi = \frac{-k}{k' - k},$$

then the sphere in question is the point the co-ordinates whereof are

$$x = \frac{k'a - ka'}{k' - k}, \quad y = \frac{k'b - kb'}{k' - k}, \quad z = \frac{k'c - kc'}{k' - k}$$

which point is the vertex of the cone: it hence appears that, taking the upper signs, the cone is the *umbral* cone, having its vertex on this side of the Moon; and similarly taking the lower signs, then if we have $\theta k - \varphi k' = 0$, that is

$$\theta = \frac{k'}{k' + k}, \quad \varphi = \frac{k}{k' + k},$$

then the variable sphere will be the point the co-ordinates of which are

$$\frac{k'a + ka'}{k' + k}, \quad \frac{k'b + kb'}{k' + k}, \quad \frac{kc' + k'c}{k' + k},$$

which point is the vertex of the cone; viz. the cone is here, the penumbral cone having its vertex between the Sun and Moon.

Taking as unity the Earth's equatorial radius, if p, p' are the

parallaxes, ϖ, ϖ' the angular semi-diameters of the Moon and Sun respectively, then the distances are $\frac{1}{\sin p}$, $\frac{1}{\sin p'}$ and the radii are $\frac{\sin \varpi}{\sin p}$, $\frac{\sin \varpi'}{\sin p'}$ respectively; hence, if h, h' are the hour-angles west from Greenwich, Δ, Δ' the N.P.D.'s of the Moon and Sun respectively, we have

$$a = \frac{1}{\sin p} \sin \Delta \cos h, \quad a' = \frac{1}{\sin p'} \sin \Delta' \cos h',$$

$$b = \frac{1}{\sin p} \sin \Delta \sin h, \quad b' = \frac{1}{\sin p'} \sin \Delta' \sin h',$$

$$c = \frac{1}{\sin p} \cos \Delta \quad , \quad c' = \frac{1}{\sin p'} \cos \Delta',$$

$$k = \frac{\sin \varpi}{\sin p} \quad , \quad k' = \frac{\sin \varpi'}{\sin p'}.$$

And thence

$$\varrho = \frac{1}{\sin^2 p} (1 - \sin^2 \varpi'),$$

$$\varrho' = \frac{1}{\sin^2 p'} (1 - \sin^2 \varpi'),$$

$$\sigma = \frac{1}{\sin p \sin p'} [\cos \Delta \cos \Delta' + \sin \Delta \sin \Delta' \cos (h' - h) \mp \sin \varpi \sin \varpi'],$$

$$\mathrm{P} = \frac{1}{\sin p} \{\sin \Delta (x \cos h + y \sin h) + z \cos \Delta\},$$

$$\mathrm{P}' = \frac{1}{\sin p'} \{\sin \Delta' (x \cos h' + y \sin h') + z \cos \Delta'\},$$

Moreover, if the right ascensions of the Moon and Sun are α, α' respectively, and if the R.A. of the meridian of Greenwich (or sidereal time in angular measure) be $= \Sigma$, then we have

$$h = \Sigma - \alpha, \quad h' = \Sigma - \alpha'.$$

It is to be observed that $h - h'$, Δ, Δ' are slowly varying quantities, viz., their variation depends upon the variation of the celestial positions of the Sun and Moon; but h and h' depend on the diurnal motion, thus varying about 15° per hour; to put in evidence the rate of variation of the several angles h, h', Δ, Δ' during the continuance of the eclipse, instead of the foregoing values of h, h', I write

$$h' = \left\{ \mathrm{E} + \left(1 + \frac{\mathrm{E}_1 - \mathrm{E}}{24}\right) t \right\} 15°,$$

there t is the Greenwich mean time, E, E_1 are the values (reckoned in parts of an hour) of the Equation of Time at the preceding and following mean noons respectively, taken positively or negatively, so that E, E_1 are the mean times of the two successive apparent noons respectively; whence also

$$h = \left\{ E + \left(1 + \frac{E_1 - E}{24} \right) t \right\} 15^\circ - \alpha + \alpha'.$$

And moreover

$$\begin{aligned} \alpha &= A + m\,(t - T), \\ \alpha' &= A + m'\,(t - T), \\ \Delta &= D + n\,(t - T), \\ \Delta' &= D' + n'\,(t - T), \end{aligned}$$

if T be the time of conjunction, A, A, D, D′ the values at that instant of the R.A.'s and N.P.D.'s; m, m' and n, n' the horary motions in R.A. and N.P.D. respectively.

It appears to me not impossible but that the foregoing form of equation

$$(x^2 + y^2 + z^2)(\varrho + \varrho' - 2\sigma) - (P - P')^2 - 2(\varrho' - \sigma)P - 2(\varrho - \sigma)P' + \varrho\varrho' - \sigma^2 = 0$$

for the umbral or penumbral cone might present some advantage in reference to the calculation of the phenomena of an eclipse over the Earth generally: but in order to obtain in the most simple manner the equation of the same cone referred to a set of principal axes, I proceed as follows :—

Writing

$$\begin{aligned} a &= b c' - b' c, & f &= a - a', \\ b &= c a' - c' a, & g &= b - b', \\ c &= a b' - a' b, & h &= c - c', \end{aligned}$$

(and therefore $af + bg + ch = 0$)

Then, if

$$X = \frac{(bh - cg)x + (cf - ah)y + (ag - bf)z}{\sqrt{a^2 + b^2 + c^2}\ \sqrt{f^2 + g^2 + h^2}},$$

$$Y = \frac{ax + by + cz}{\sqrt{a^2 + b^2 + c^2}},$$

$$Z = \frac{fx + gy + hz}{\sqrt{f^2 + g^2 + h^2}},$$

X, Y, Z, will be co-ordinates referring to a new set of rectangular axes; viz., the origin is, as before, at the centre of the Earth, the axis of Z is parallel to the line joining the centres of the Sun and Moon; the axis of X cuts at right angles the last-mentioned line; and the axis of Y is perpendicular to the plane

of the other two axes; or, what is the same thing, to the plane through the centres of the Earth, Sun, and Moon.

The co-ordinates of the vertex of the cone are therefore X_0, Y_0, Z_0, where these denote what the foregoing values of X, Y, Z, become on substituting therein for x, y, z, the values

$$\frac{k'a \mp ka'}{k' \mp k}, \qquad \frac{k'b \mp kb'}{k' \mp k}, \qquad \frac{k'c \mp kc'}{k' \mp k},$$

and the equation of the cone therefore is

$$(X - X_0)^2 + (Y - Y_0)^2 = \tan^2 \lambda\, (Z - Z_0)^2,$$

where

$$\sin \lambda = \frac{k' \mp k}{G}$$

if for a moment G denotes the distance between the centres of the Sun and Moon. We have therefore

$$\tan \lambda = \frac{k' \mp k}{\sqrt{G^2 - (k' \mp k)^2}},$$

or since

$$G^2 = (a' - a)^2 + (b' - b)^2 + (c' - c)^2,$$

this is in fact

$$\tan \lambda = \frac{k' \mp k}{\sqrt{\varrho + \varrho' - 2\sigma}},$$

where ϱ, ϱ', σ signify as before; and thus X_0, Y_0, Z_0, $\tan \lambda$ are all of them given functions of a, b, c, k, a', b', c', k', and consequently of the before-mentioned astronomical data of the problem. The form is substantially the same as Bessel's equation (3), *Ast. Nach.* No. 321 (1837), (but the direction of the axes of X, Y, is not identical with those of his x, y); and it is therefore unnecessary to consider here the application of it to the calculation of the eclipse for a given point on the Earth.

The Astronomische Gesellschaft.

The third biennial meeting was held at Vienna from the 13th to the 16th of September, 1869, under the presidency of M. Struve; the number of members present was 39; total number 216. The several subjects in question at the meeting of 1867 (*See Monthly Notices*, vol. xxviii. p. 268) were further discussed, and other subjects brought before the meeting; the principal ones were as follows:—

1. As to the New Tables of *Jupiter*, a large part of the

auxiliary calculations had been performed under the direction of Prof. Hansen, by junior members of the Society; and (his own part of the work being already accomplished) it was hoped that in the following year the definitive amendment of the elements could be taken in hand.

2. The reduction of the observations of the Periodic Comets had been proceeded with; a desideratum in reference thereto had been supplied by the completion of the *Tabulæ Quantitatum Besselianarum pro Annis* 1750 *ad* 1840, under the direction of MM. Struve and Auwers.

3. In reference to the new reduction of Bradley's observations, Prof. Auwers reported that the more mechanical part of the work was already accomplished. The number of zenith distances observed between 1750 and 1762 (exclusively of those of the Moon and planets, which were not in the first instance to be included in the new reduction) was about 19,000, of which 1650 related to fundamental stars.

4. Dr. Schmidt, Director of the Observatory at Athens, exhibited eight sheets of his new map of the Moon; the plan being that of Lohrman's, but the scale about double, the diameter being 6 Paris feet. It was stated that the publication of the map would hardly be completed within 10 years.

5. Plans were exhibited of the proposed New Observatory at Vienna.

6. In reference to the plan for the observation of the stars of the northern hemisphere up to the ninth magnitude, reports were received from MM. Struve, Kowalski, Krüger, Schwarz, Bruhns, Tiele, Auwers, and Safford, upon the progress which had been made: and a definitive programme was drawn up, which is published in the *Vierteljahrschrift.* It appears thereby that the work has been undertaken by the different observatories in zones as follows:—

80°	to	75°	Kasan.
75	„	70	Dorpat.
70	„	65	Christiania.
65	„	55	Helsingfors.
55	„	50	*Vacant.*
50	„	40	Bonn.
40	„	35	Chicago.
35°	„	30°	Leipzig.
30	„	25	Cambridge (E.)
25	„	15	Berlin.
15	„	10	Leipzig.
10	„	4	Mannheim.
4	„	1	Neuchatel.
+ 1	„	− 2	Palermo.

7. Prof. Förster presented a report on the Eclipse-expedition

of 1868, which was intended to be published as a supplement to the *Vierteljahrschrift.*

8. Some discussion took place as to the proposed expedition for the observation of the transit of 1874.

The next meeting was fixed to be at Stuttgard, in 1871, under the presidency of M. Struve.

Besides the *Vierteljahrschrift* and the several works referred to *Monthly Notices*, vol. xxviii., there have been published by the Society.

Supplement-heft zu Jahrgang III.. von Asten Dr. E. *Neue Hulfstafeln zur Reduction der in der Histoire Céleste Française enthaltenen Beobachtungen*, 1868.

9. Lesser Dr. Otto. *Tafeln der Pomona mit Berücksichtigung der Störungen durch Jupiter, Saturn und Mars berechnet*, 1869.

Instrument for Sale.

A large spectroscope, recently constructed by Messrs. Simms for Mr. Huggins. The prisms are by Hofmann. It is adapted for use with the telescope, and suitable for observation of nebulæ, stars, and the Sun. It can be used for table purposes. To be seen at Mr. Ladd's, Beak Street, Regent Street, W.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *May* 13, 1870. No. 7.

WILLIAM LASSELL, Esq., President, in the Chair.

Edward Dent, Esq., 12 Hyde Park Gardens;
James Dyson Perrins, Esq., Davenham Bank, Malvern; and
Charles Henry Gatty, Esq., Felbridge Park, East Grinsted,
were balloted for and duly elected Fellows of the Society.

Meteorological Observations taken at Gibraltar.

By Lieut. Alexander B. Brown, R. A.

Since some discussion has been entered into with respect to a suitable station for observing the total phase of the Solar Eclipse of December 22nd, 1870; and as it is absolutely necessary for the satisfactory carrying out of our experiments that we should have a clear or moderately clear sky, I have much pleasure in submitting to the Society some extracts from my meteorological notes taken in Gibraltar from 1860 to 1867. From that date (1867) to the present time I have fallen back (for the last two years) to well authenticated observations.

I should remark that at Gibraltar,—

b means blue sky, quite or almost quite free from cloud;
c means much or little blue sky with cloud;
f, fog; *m*, mist; *g*, gloomy; *r*, rain; *p*, passing showers.

As respects force of wind,—

1, light air; 2, light breeze;
3, gentle breeze; 4, moderate breeze;
5, fresh breeze; 12, hurricane.

Notes on 22nd December.

	Wind.	Force.	Barometer.	Sky.
1860, Dec. 22	SW, W	6	29·93	Fog
1861, ,,	SW, SSW	5	29·78	Showery
1862, ,,	WNW	3	30·01	Blue sky
1863, ,,	NE, WNW	2	30·23	Blue sky
1864, ,,	ENE, NE	2	30·00	Blue sky, with cloud
1865, ,,	SE, ESE	4	30·25	Cloudy
1866, ,,	E	3	30·24	Blue sky, with cloud
1867, ,,	W	4	30·16	Blue sky
1868, ,,	WSW, SW	5	30·13	Blue sky
1869, ,,	WNW	5	30·07	Blue sky, with cloud

Meteorological Notes of 17 Days, from 15 to 31 December (inclusive) for past 10 years.

17 Days. 15–31 December.	Average Wind.	Force of Wind.	*b.*	*c.*	*f, m.*	*g.*	*r.*	*p.*
1860	WNW, SW	5	6	6	4	1	..	..
1861	NE, SE	4	3	5	1	1	6	1
1862	WNW, ENE	3	10	6	1	..	..	..
1863	WNW	3	13	3	..	..	1	..
1864	WNW	4	5	10	1	..	..	1
1865	E, ENE, ESE	2	12	4½	..	..	½	..
1866	E, ESE	4	6	9	2	..	..	..
1867	W, WNW	3	8	5	1	..	½	2½
1868	WNW	5	11	5	..	..	1	..
1869	WNW	5	7	9	½	..	..	½

The numbers in the last six columns show for each year how many of the 17 days were *b*, *c*; *f*, *m*; *g*, *r*, or *p*, respectively; the total in each line being of course = 17. The observations being taken at 9 A.M. and 3 P.M., ½ days have been introduced.

We thus see then, out of the last 10 returns of 22nd December, there were

4 very good observing days;
3 fair ditto;
1 not quite so good;
1 in which probably little could be done; and
1 useless for observation of the Sun.

That from the 15th to 31st December in the

Years 1860	6 very good days		6 fair days		5 indifferent	
1861	3	,,	5	,,	9	,,
1862	10	,,	6	,,	1	,,
1863	13	,,	3	,,	1	,,

Years						
1864	5	very good days	10	fair days	2	indifferent
1865	12	„	4½	„	½	„
1866	6	„	9	„	2	„
1867	8	„	5	„	3	„
1868	11	„	5	„	1	„
1869	7	„	9	„	1	„

which I think we may consider very satisfactory.

On his Photographs taken during the Total Solar Eclipse, Aug. 7, 1869. By Commander Ashe.

(*Letter addressed to the Editor of the Monthly Notices.*)

In the *Monthly Notices* of the Astronomical Society of Feb. 11, 1870, in the Council Report, at page 108 (in reference to the four negatives taken during totality at Jefferson Town) are these words,—"Unfortunately, in photographs 3 and 4, there is evidence of the disturbance of the telescope during the exposure of the sensitive plate."

Now I beg to state there is no evidence of the sort, and by the enclosed print of an enlarged copy of No. iv. I clearly prove that no disturbance of the telescope took place. I have given ample evidence to the Astronomer Royal that the telescope was not disturbed; but if there was nothing else, the photograms taken at Des Moines (the next station to us), would prove it, for, in the photogram taken near the end of totality, are seen the stumps of the three parallel planes of light that are seen in No. iv. photogram at E. My negative was taken as near the limb of the Sun as it is possible to take it, for just as I closed the slide the Sun burst out, and this remarkable photogram shows two bright bands divided by a dark band. What are they? They are crossed by rays of light that do not radiate from the Sun's centre, but are all parallel to each other. It must be remembered that no other party was looking at the totality at the same time that we were, for just as it ended at Jefferson it commenced at Des Moines.

Now, with reference to my photograms "confirming what has already been stated in regard to the identity of form preserved by the protuberances," I will now relate a fact that was seen by all at Jefferson, and by Mr. Falconer, whose address is Alexander Plytts Falconer, Esq., Bath County Club, Bath, and may be referred to. The point of light that is seen in No. i., and is described by all the American astronomers, on the "great protuberance"—whilst I was waiting for No. iii. plate—shot out to an enormous length, at least one-third longer than it is seen in No. iii. photogram; and when it reached its greatest height (which

it did in a few seconds), the top was blown off at right angles and came to a point, just like a flame acted on by a blow-pipe. The lower part was deep red, getting lighter, and the part blown off was a brilliant white light. Now then let us investigate the evidence. Although this is my second total eclipse, still my surprise and love of the marvellous might be supposed to have produced in my bewildered mind an impression that did not exist in reality. If I were the only one who saw it, then it might very readily be put down to fancy, but all those who came out from the town, and were standing round the observatory, saw it, and the crowd produced that sensational murmur that they did when the totality commenced. One man described it as a crooked piece of iron taken from a forge "white hot" at the top. But Mr. Falconer, who was then travelling in America, gave me a very good drawing of it, and who no doubt will, if written to, corroborate my statement.



Now I state that a few seconds after it had reached its greatest height, it lost the flame-like appearance, and became a duller red, and quickly reduced its height; and when No. iii. negative was taken, it was apparently a cinder; and when this negative is examined by a lens, it shows cracks and various lines, that were also seen by Mr. Vail, who observed the eclipse with a small telescope. Now, supposing that we *all* agreed to foist this marvellous story on the public, still there is the photogram No. iii., which shows an enormous protuberance, which was fast crumbling away; and on examining No. iv. negative, we see it greatly reduced; but still it has the same characteristic form. Now look at the "Des Moines" photogram. Here we see that it has assumed the form of a great heap of cinders, but the long exposure, 66 seconds, has softened the outlines.

After the protuberance has taken this form, then it naturally retains it for some time, and thus all the other stations to the eastward have very similar photograms.

In common justice to our party I have to request that this communication may be printed in the *Monthly Notices* of the Society. And, in conclusion, I beg that the negatives may be returned to me, as I am about to print my Report.

Observatory, Quebec, April 14, 1870.

A Committee appointed by the Council unanimously report that, in their opinion, there was a decided movement of the instrument at the time the photograph was taken. This conclusion they arrived at from an examination of the chromosphere close to the Moon's limb, as well as from an examination of the prominences.

Observations of an Occultation of Saturn by the Moon; of Occultations of Stars by the Moon; and of Phenomena of Jupiter's Satellites; made at the Royal Observatory, Greenwich, from 1869, *April, to* 1870, *April.*

(*Communicated by the Astronomer Royal.*)

Occultation of Saturn (*Disappearance and Reappearance*), 1870, *April* 19.

Phenomenon.	Greenwich Mean Solar Time of Observation. Hour and Minute.	Seconds observed by E.	C.	J. C.
	h m	s	s	s
First Contact with Ring	14 55	49·8	55·5	50·8
First Contact with Ball	14 56	13·8	17·4	16·7
Total Disappearance of Ball	14 56	44·7	53·3	60·1
Total Disappearance of Ring	14 57	15·6	16·8	24·6
First Reappearance of Ring	16 4	...	34·2	33·5
First Reappearance of Ball	16 5	...	0·1	0·4
Last Contact with Ball	16 5	...	40·5	44·3
Last Contact with Ring	16 6	...	5·4	6·2

Notes by E.—With the Altazimuth and a power of about 100. The first three times probably uncertain to 1ˢ or 2ˢ; the last still more uncertain, the light of the planet by contrast with the Moon being extremely faint.

By C.—With the Sheepshanks Equatoreal and a power of 64. The observations of the disappearance were not considered good, owing to the great faintness of the planet on approaching the bright limb of the Moon; the first contact with the limb of *Saturn* being the best. The observations of the reappearance were considered pretty good; those of the first limb of the planet and last contact of ring being thought the most accurate. There was not the least trace of alteration of the planet's form noticed; in fact, the Moon's limb at the reappearance seemed to divide the planet with extraordinary sharpness. Owing to the amount of colour on *Saturn*, in order to get a good image I reduced the aperture of the object-glass to 4¼ inches; this gave a very nice image indeed.

By J. C.—With the Great Equatoreal and a power of 340. At disappearance the planet was a very dull object when in contact with the Moon; its light probably a twentieth as bright. The times noted are probably correct to a second, except the last, which is doubtful to two or three seconds. At reappearance the planet was rather tremulous; no distortion whatever was noticed.

Occultations of Stars by the Moon.

Day of Observation.	Phenomenon.	Moon's Limb.	Mean Solar Time.	Observer.
1869.			h m s.	
June 23	(*a*) μ^1 Sagittarii, disappearance	Bright	10 53 22·5	D.
Aug. 2	Aldebaran, reappearance	Dark	13 13 39·8	J. C.
Dec. 14	ξ^2 Ceti, disappearance	Dark	9 20 17·5	E.
1870.				
Feb. 10	m Tauri, disappearance	Dark	9 8 16·3	E.

(*a*) The star very faint at disappearance.

Phenomena of Jupiter's Satellites.

Day of Obs.	Satellite.	Phenomena.	Mean Solar Time. h m s	Observer.
1869. Oct. 5	I.	Eclipse, disappearance	12 44 55·9	H.C.
21	III.	Eclipse, disappearance	8 50 45·6	C.
	III.	Eclipse, reappearance	10 32 43·9	C.
	III.	(*a*) Occult. disappearance, bisection	10 55 10·2	C.
	I.	Eclipse, disappearance	11 2 59·0	C.
Nov. 15	III.	Transit, ingress, first contact	10 13 16·0	J.C.
	III.	,, ,, bisection	10 21 44·6	J.C.
	III.	Transit, egress, bisection	11 51 29·8	J.C.
	III.	,, ,, last contact	11 57 58·7	J.C.
19	II.	Eclipse, reappearance	13 17 37·2	J.C.
Dec. 14	II.	Eclipse, reappearance	10 22 31·7	E.
15	I.	Eclipse, reappearance	10 3 21·1	D.
1870. Jan. 30	I.	(*b*) Eclipse, reappearance	10 37 24·5	S.
Feb. 27	III.	Eclipse, disappearance	9 23 30·0	S.
Mar. 13	II.	(*c*) Eclipse, reappearance	6 52 17·7	J.C.
Apr. 4	III.	(*d*) Eclipse, reappearance	7 18 26·3	E.

(*a*) Owing to the path of the satellite cutting the disc of the planet at a very small chord, and the occultation being little more than a graze, it was impossible to estimate with any accuracy the time of occultation.

(*b*) A haze prevalent; the time noted probably somewhat late.

(*c*) The sky rather bright from daylight.

(*d*) Very faint; the time noted is that at which the satellite was first seen; it could not have been visible more that a few seconds previously.

The initials S., D., E., C., J. C., and H. C. are those of Mr. Stone, Mr. Dunkin, Mr. Ellis, Mr. Criswick, Mr. Carpenter, and Mr. H. Carpenter.

Occultation of Saturn by the Moon, Tuesday, April 19, 1870. — By Capt. W. Noble.

As this was my first view of *Saturn* this year I occupied myself, from $14^h\ 40^m$ L.M.T., in scrutinising the physical features of the planet before the occultation. I employed a power of 255 on my 4·2-inch Equatoreal, the same with which I subsequently observed the occultation itself.

Notwithstanding *Saturn's* small altitude he was well and sharply defined, Ball's division being visible over the North Pole. The shadow of the ball was of course to the west of it on the rings. The crape ring C was seen in the ansæ very distinctly. *Saturn* appeared of a richly greenish yellow when compared with the brilliant white light of the Moon.

The Occultation.

The first contact of the outer edge of ring A with the Moon's bright limb took place at $16^h\,47^m\,55^s$ L.S.T. $= 14^h\,55^m\,55^s{\cdot}6$ L.M.T. and that of the inner edge of ring B at $16^h\,48^m\,8^s{\cdot}6$ L.S.T. $= 14^h\,56^m\,9^s{\cdot}2$ L.M.T. The preceding limb of the planet touched that of the Moon at $16^h\,48^m\,19^s$ L.S.T. $= 14^h\,56^m\,19^s{\cdot}6$ L.M.T. The globe of *Saturn* was dichotomised (as nearly as I could estimate) at $16^h\,48^m\,33^s$ L.S.T. $= 14^h\,56^m\,33^s{\cdot}5$ L.M.T. His following limb disappeared at $16^h\,49^m\,1^s$ L.S.T. $= 14^h\,57^m\,1^s{\cdot}4$ L.M.T. The inner edge of ring B was occulted at $16^h\,49^m\,13^s$ L.S.T. $= 14^h\,57^m\,13^s{\cdot}4$ L.M.T., and the last perceptible trace of the ring vanished at $16^h\,49^m\,25^s$ L.S.T. $= 14^h\,57^m\,26^s{\cdot}4$ L.M.T. Although very pale the planet was perfectly distinct, and passed behind the Moon's limb without wave, shake, or distortion.

At the reappearance of *Saturn* the first visible trace of the edge of the preceding ansa was caught sight of about $17^h\,57^m\,19^s$ L.S.T. $= 16^h\,5^m\,8^s{\cdot}3$ L.M.T., and at $17^h\,58^m\,1^s$ L.S.T. $= 16^h\,5^m\,50^s{\cdot}1$ L.M.T. the planet was *just* clear of the Moon's limb.

The emersion was very striking, from the exceeding sharpness of *Saturn*; the most delicate detail being perceptible, even in contact with the lunar limb. The crape ring C was seen most perfectly where the dark limb of the Moon crossed it. I never was more impressed with the absolute absence of a lunar atmosphere of any appreciable density than I was on this occasion.

Forest Lodge, Maresfield, Sussex,
May 13, 1870.

Occultation of Saturn. By C. G. Talmage, Esq.

The occultation of *Saturn* was well seen here: at sunset the sky was quite clear, and remained so to sunrise. *Saturn* was visible to the naked eye to within three minutes and a-half of the time of disappearance.

When I first looked at *Saturn*, at about 13^h, no striking difference of colour from the Moon was visible, but by 14^h the difference was quite perceptible, and at $14^h\,45^m$ it was most marked, the planet appearing of a yellow tint.

I had no difficulty whatever in observing both the disappearance and reappearance of *Titan*. To prevent the glare of the Moon I covered the eyepiece half over with silver foil, so that the eye was greatly relieved. The field was, therefore, of the following shape:—



The local mean times are as follows:—

	h	m	s
Disappearance of *Titan* =	14	49	18·97
First contact with ring	14	55	51·90
,, ,, ball	14	56	15·84
Final disappearance of ball	14	56	57·73
,, ,, ring	14	57	17·68
Reappearance of *Titan*	15	58	56·53
,, ,, ring	16	4	31·63
,, ,, ball	16	4	58·56
Ring clear of Moon =	16	6	1·89

I used a power of 110 on the 10-inch refractor.

I believe, had I taken the precaution of getting out the positions of the faint satellites, I could have observed their occultations, as I saw several very faint objects occulted, both preceding and following *Saturn*, and the field was full of stars.

The position of the Observatory is—

Lat. 51° 34′ 34″ N.
Long. 0^h 0^m 0^s·87 W.

Mr. Barclay's Observatory, Leyton.

Occultation of Saturn. By John Joynson, Esq.

The occultation of *Saturn* by the Moon, on the 19th April last, was seen here as satisfactorily as could be expected, considering the low altitude of the planet. The sky was quite free from cloud at the disappearance, but at the reappearance there was just sufficient hazy cloud about the Moon to interfere with the correct observation of the exact time of the egress of the preceding edge of the ring.

The following were the times noted:—

Disappearance.

	h	m	s	
First contact of ring	14	52	52·7	G.M.T.
Inner ditto ditto		53	2·2	
First ditto of ball		53	22·1	
Last ditto ditto		53	39·6	
Inner ditto of ring		54	2·0	
Last ditto ditto	14	54	10·1	

Reappearance.

First appearance of ring.	Not noted, for hazy cloud.
Inner edge ditto	Not sure ditto

	h	m	s	
First limb of ball	16	0	29·1	G.M.T.
Last ditto ditto		0	46·0	
Inner edge of ring		1	7·3	
Last ditto ditto	16	1	15·9	

Lens $3\frac{1}{2}$ inches; Power 110.

Lat. 53° 28′ 24″ N.
Long. $0^h\ 12^m\ 6^s{\cdot}9$ W.

Waterloo, near Liverpool,
May 10, 1870.

Observations of Algol; of Occultations of Stars and of Saturn by the Moon; and of Sun-spots. By C. F. Penrose, Esq.

The position is

Latitude 51° 24′ 58 N.
Longitude $0^h\ 0^m\ 55{\cdot}1$ W.

Algol.

			G.M.T.
Minima observed by estimation	1869	Oct. 11	$10^h\ 50^m$
	and	Dec. 18	$6^h{\cdot}7$

These observations, which represent the phenomena certainly within 10 minutes, show that the period of 2·86727 days which has been assigned, combined with an epoch given for Jan. 3, 1844, in the *Outlines of Astronomy*, requires a slight correction. These minima occurred nearly 3 hours earlier than if due to those data. The value 2·867234 would better satisfy them.

Occultations as follows:—

1869, Aug. 2.

			G.M.T.
Aldebaran	Reapp.	Dark limb	$13^h\ 13^m\ 37^s$

1870, Feb. 2.

π Tauri	Disapp.	Dark limb	$9^h\ 7^m\ 14^s{\cdot}9$

April 19th.

Saturn.

	At Disapp.	Bright limb	G.M.T. h m s
First contact of Ring		..	14 55 30
Ball apparently bisected		..	56 17
Final disappearance of Ring		..	57 5

At Reapp.	Dark limb	h	m	s
Ball bisected ..	..	16	5	2
Ring clear of Moon	..		5	52

Of these occultations the two first have been subjected to calculation. That of *Aldebaran* accords with the longitude within a very few seconds. In the case of *m Tauri* the discordance is greater (viz. about 13ˢ), but the occultation was far from a central one. The Moon seems to have been behind, or above, or both, as repects her tabular place.

Several Sun-spots have been noticed exhibiting a remarkable appearance when near the limb, especially on March 25 and April 25, a sketch of the latter is submitted.

If the Sun-spots are cuplike or conical depressions and symmetrically placed, or nearly so, with respect to a normal or solar plumbline, the breadth of the nearer margin would invariably be less than that of the further margin when near the limb, and would even disappear on approaching it (which is the general phenomenon). That, of which the sketch is submitted, on April 25, was about 25° from the limb, and exhibited its nearer margin equal in breadth to the further one. By the imaginary section through the photosphere it is shown how very oblique must have been the axis of the cavity around the spot on the hypothesis of its cuplike shape.

Colebyfield, Wimbledon.

Note respecting η Argûs. By H. A. Severn, Esq.

(*Extract of Letter addressed to the Astronomer Royal by Henry A. Severn, Union Bank of Australia, Melbourne, Victoria, received April* 1870.)

"I may say that I cannot confirm the new position given to η *Argûs* in respect to the Nebula. I have watched it for 14 years, and it is just where it was; of course much less brilliant."

Instruments, 13 in. front view reflector, of his own construction, and a $3\frac{3}{4}$ in. refractor.

Comparisons of the Places of certain Stars, as given in the Second Radcliffe Catalogue, with the Places given by Dr. Wolfers in the Tabulæ Reductionum. By Dr. Wolfers. Translation.

Extract of a Letter from Dr. Wolfers to the Radcliffe Observer.

"I beg to offer my best thanks for the copy which I have received of the Second Radcliffe Catalogue of Stars, presented to me by the Radcliffe Trustees.

" You will not be surprised that I have, as on former occasions, compared the mean places of the stars therein contained with the places determined by me, since the latter are now in use at several observatories. The agreement is, as in all former comparisons, in general very satisfactory, as you will see by the accompanying copy. I must leave it to your own judgment to determine whether it is proper to communicate the paper to the Royal Astronomical Society. It will be a satisfaction to me if, by this means, the use of the *Tabulæ Reductionum* should be extended.

Comparison of Fundamental Stars (Tab. Red. = W.) *with the Second Radcliffe Catalogue* (M.)

Name.	No. of Obs.	R.A. M.	R.A. W.	R.A. M.—W.	No. of Obs.	Declination. M.	Declination. W.	Declination. M.—W.
		h m s	s	s		° ′ ″	″	″
Andromedæ	12	0 1 9·38	9·43	−0·05	7	+28 19 3·7	3·3	+0·4
Pegasi	21	0 6 1·77	1·86	−0·09	6	+14 24 21·0	18·5	+2·5
Cassiopeiæ	3	0 32 35·14	35·06	+0·08	6	+55 46 7·3	8·4	−1·1
Arietis	14	1 59 17·25	17·32	−0·07	2	+22 47 56·4	55·2	+1·2
Ceti	18	2 54 57·89	57·87	+0·02	4	+ 3 32 17·4	17·0	+0·4
Persei	5	3 14 20·74	20·80	−0·06	4	+49 21 33·0	33·2	−0·2
Tauri	19	4 27 53·43	53·46	−0·03	11	+16 13 28·4	28·4	0·0
Aurigæ	10	5 6 21·06	21·17	−0·11	9	+45 51 2·8	3·4	−0·6
Orionis	11	5 7 48·62	48·70	−0·08	9	− 8 21 59·2	59·5	+0·3
Tauri	7	5 17 26·69	26·67	+0·02	7	+28 29 8·2	5·9	+2·3
Orionis	13	5 47 35·58	35·64	−0·06	11	+ 7 22 38·8	38·2	+0·6
Canis Maj.	10	6 38 58·63	58·70	−0·07	24	−16 31 36·6	36·2	−0·4
Geminorum	13	7 25 39·52	39·45	+0·07	15	+32 11 30·5	29·4	+1·1
Canis Min.	15	7 31 58·35	58·36	−0·01	12	+ 5 34 50·8	50·5	+0·3
Geminorum	17	7 36 44·64	44·69	−0·05	11	+28 21 39·7	38·9	+0·8
Hydræ	15	9 20 42·43	42·47	−0·04	8	− 8 3 15·0	12·9	−2·1
Leonis	20	10 0 54·71	54·78	−0·07	16	+12 38 60·6	59·9	+0·7
Ursæ Maj.	5	10 55 3·51	3·28	+0·23	16	+62 30 21·4	20·7	+0·7
Leonis	15	11 41 54·98	55·00	−0·02	6	+15 21 16·1	16·8	−0·7
Virginis	11	11 43 24·20	24·16	+0·04	5	+ 2 33 12·3	12·9	−0·6
Ursæ Maj.	6	11 46 27·25	27·08	+0·17	4	+54 28 22·8	23·2	−0·4
Virginis	21	13 17 49·29	49·32	−0·03	18	−10 25 46·8	45·3	−1·5
Ursæ Maj.	13	13 42 1·27	1·25	+0·02	7	+50 0 46·6	47·8	−1·2
Boötis	11	14 9 16·55	16·62	−0·07	5	+19 54 47·2	47·5	−0·3
Libræ	1	14 42 56·89	56·92	−0·03	1	−15 24 47·1	44·9	−2·2
Libræ	14	14 43 8·32	8·36	−0·04	3	−15 27 27·8	26·5	−1·3
Ursæ Min.	1	14 51 8·89	9·23	−0·34	7	+74 43 38·8	38·3	+0·5
Coronæ	14	15 28 45·67	45·71	−0·04	5	+27 11 17·5	18·2	−0·7
Serpentis	15	15 37 22·43	22·48	−0·05	4	+ 6 52 7·0	8·1	−1·1
Scorpii	18	16 20 49·70	49·73	−0·03	10	−26 7 2·9	2·4	−0·5
Herculis	11	17 8 15·87	15·95	−0·08	2	+14 33 11·0	11·0	0·0
Ophiuchi	26	17 28 26·13	26·23	−0·10	11	+12 39 54·6	54·8	−0·2

Name.	No. of Obs.	R.A. M.	R.A. W.	R.A. M.—W.	No. of Obs.	Declination. M.	Declination. W.	M.
		h m s	s	s		° ′ ″	″	
γ Draconis	20	17 53 21·35	21·47	−0·12	6	+51 30 24·8	24·5	
α Lyræ	14	18 32 11·89	11·94	−0·05	14	+38 39 20·4	20·2	
γ Aquilæ	21	19 39 36·13	36·25	−0·12	9	+10 16 29·3	29·8	
α ,,	26	19 43 57·08	57·18	−0·10	15	+ 8 30 5·5	5·5	
β ,,	20	19 48 26·19	26·21	−0·02	8	+ 6 3 34·9	35·6	
α^1 Capricorni	1	20 9 53·16	53·17	−0·01	4	−12 56 18·7	15·9	
α ,,	14	20 10 17·08	17·09	−0·01	7	−12 58 35·2	32·6	
α Cygni	26	20 36 39·47	39·59	−0·12	12	+44 46 52·7	54·2	
α Cephei	17	21 15 14·18	14·14	+0·04	20	+61 59 35·2	35·0	
α Aquarii	25	21 58 35·49	35·57	−0·08	6	− 0 59 55·1	54·0	
α Piscis Austr.	10	22 49 54·40	54·44	−0·04	11	−30 21 47·2	47·5	
α Pegasi	22	22 57 47·33	47·39	−0·06	9	+14 27 9·3	10·9	
α Ursæ Min.	14	1 8 2·37	2·65	−0·28	17	+88 33 46·9	47·1	
δ Ursæ Min.	4	18 17 30·60	30·16	+0·44	17	+86 36 7·4	6·2	

Comparison of Additional Stars (*Astr. Jahrbuch*, 1867, page 325, = (V *with the Second Radcliffe Catalogue* (M).

Name.	No. of Obs.	R.A. M.	R.A. W.	R.A. M.—W.	No. of Obs.	Declination. M.	Declination. W.	M.
		h m s	s	s		° ′ ″	″	
β Ceti	13	0 36 33·63	33·63	0·00	5	−18 45 21·9	21·3	
γ ,,	18	2 36 3·01	3·01	0·00	7	+ 2 38 37·8	37·1	
δ Arietis	17	3 3 37·74	37·82	−0·08	8	+19 11 40·3	40·1	
ι Ursæ Maj.	10	8 49 36·36	36·32	+0·04	9	+48 35 17·7	17·6	
θ ,,	4	9 23 28·32	28·28	+0·04	1	+52 18 48·4	46·3	
γ^1 Leonis	9	10 12 14·91	14·95	−0·04	4	+20 32 54·0	53·7	
χ ,,	13	10 57 47·60	47·70	−0·10	9	+ 8 5 30·7	30·9	
δ ,,	10	11 6 39·39	39·58	−0·19	8	+21 17 25·8	25·0	
δ Crateris	9	11 12 20·59	20·67	−0·08	7	−14 1 18·1	17·2	
γ Virginis med.	9	12 34 34·11	34·13	−0·02	4	− 0 40 53·3	51·0	
12^2 Canum Ven.	7	12 49 28·47	28·46	+0·01	3	+39 4 31·2	31·1	
ζ Virginis	11	13 27 33·58	33·82	−0·24	5	+ 0 7 14·9	17·9	
η Boötis	17	13 48 1·15	1·22	−0·07	6	+19 6 3·5	4·0	
ψ ,,	7	14 58 26·80	26·86	−0·06	4	+27 29 43·9	45·2	
ζ Ursæ Min.	2	15 49 8·72	8·53	+0·19	4	+78 13 27·5	24·0	
ζ Herculis	16	16 36 0·57	0·72	−0·15	7	+31 51 32·2	30·7	
κ Ophiuchi	17	16 51 2·53	2·56	−0·03	7	+ 9 35 44·0	45·3	
β Draconis	12	17 27 16·12	16·27	−0·15	15	+52 24 23·8	23·4	
μ Herculis	22	17 40 58·78	58·78	0·00	10	+27 48 18·9	19·9	
β^1 Lyræ	26	18 44 54·61	54·61	0·00	4	+33 12 9·6	9·0	
ι Aquilæ	27	19 18 26·30	26·34	−0·04	8	+ 2 50 19·8	19·9	
61^1 Cygni	18	21 0 37·43	37·39	+0·04	12	+38 3 47·8	47·3	
γ Piscium	22	23 9 54·42	54·47	−0·05	6	+ 2 31 3·4	4·8	
ι ,,	18	23 32 45·00	45·13	−0·13	13	+ 4 52 3·7	3 9	
ω ,,	11	23 52 7·34	7·46	−0·12	6	+ 6 5 16·1	17·9	



Note on Dr. Wolfers' Paper. Comparison between the Places of the Greenwich Seven-year Catalogue and Radcliffe Catalogue, 1860, for the Stars in Table I. By E. J. Stone, Esq.

	Greenwich Mean Right Ascension. h m s	Rad.	G.—R.	Mean N.P.D. ° ′ ″	Rad.	G.—R.
α Andromedæ	0 1 9·44	9″·38	+ 0″·06	61 40 57·79	56″·3	+ 1″·5
γ Pegasi	0 6 1·79	1·77	+ 0·02	75 35 42·37	39·0	+ 3·4
α Cassiopeiæ	0 32 35·06	35·14	− 0·08	34 13 51·94	52·7	− 0·8
α Arietis	1 59 17·28	17·25	+ 0·03	67 12 5·60	3·6	+ 2·0
α Ceti	2 54 57·83	57·89	− 0·06	86 27 42·81	42·6	+ 0·2
α Persei	3 14 20·72	20·74	− 0·02	40 38 26·72	27·0	− 0·3
α Tauri	4 27 53·43	53·43	0·00	73 46 32·97	31·6	+ 1·4
α Aurigæ	5 6 21·12	21·06	+ 0·06	44 8 57·40	57·2	+ 0·2
β Orionis	5 7 48·63	48·62	+ 0·01	98 21 59·90	59·2	+ 0·7
β Tauri	5 17 26·67	26·69	− 0·02	61 30 54·08	51·8	+ 2·3
α Orionis	5 47 35·59	35·58	+ 0·01	82 37 21·95	21·2	+ 0·8
α Canis Majoris	6 38 58·66	58·63	+ 0·03	106 31 36·23	36·6	− 0·4
α Geminorum	7 25 39·31 39·73	39·29 39·74	+ 0·02 − 0·01	57 48 33·16 30·65	31·8 29·5	+ 1·4 + 1·2
α Canis Minoris	7 31 58·34	58·35	− 0·01	84 25 9·79	9·2	+ 0·6
β Geminorum	7 36 44·68	44·64	+ 0·04	61 38 21·25	20·3	+ 1·0
α Hydræ	9 20 42·44	42·43	+ 0·01	98 3 13·76	15·0	− 1·2
α Leonis	10 0 54·78	54·71	+ 0·07	77 21 0·62	59·4	+ 1·2
α Ursæ Majoris	10 55 3·40	3·51	− 0·11	27 29 39·12	38·6	+ 0·5
β Leonis	11 41 54·98	54·98	0·00	74 38 43·96	43·9	+ 0·1
β Virginis	11 43 24·13	24·20	− 0·07	87 26 47·35	47·7	− 0·4
γ Ursæ Majoris	11 46 27·06	27·25	− 0·19	35 31 37·22	37·2	0·0
α Virginis	13 17 49·27	49·29	− 0·02	100 25 45·54	46·8	− 1·3
η Ursæ Majoris	13 42 1·27	1·27	0·00	39 59 12·81	13·4	− 0·6
α Boötis	14 9 16·62	16·55	+ 0·07	70 5 13·40	12·8	+ 0·6
δ Libræ	14 42 56·87	56·89	− 0·02	105 24 46·90	47·1	− 0·2
α Libræ	14 43 8·31	8·32	− 0·01	105 27 27·41	27·8	− 0·4
β Ursæ Minoris	14 51 9·16	8·89	+ 0·27	15 16 20·65	21·2	− 0·5
α Coronæ	15 28 45·69	45·67	+ 0·02	62 48 42·96	42·5	+ 0·5
α Serpentis	15 37 22·44	22·43	+ 0·01	83 7 52·88	53·0	− 0·1
α Scorpii	16 20 49·70	49·70	+ 0·00	116 7 2·96	2·9	+ 0·1
α² Herculis	17 8 16·24	16·23	+ 0·01	75 26 52·77	53·2	− 0·4
α Ophiuchi	17 28 26·22	26·13	+ 0·09	77 20 6·26	5·4	+ 0·9
γ Draconis	17 53 21·34	21·35	− 0·01	38 29 36·01	35·2	+ 0·8
α Lyræ	18 32 11·92	11·89	+ 0·03	51 20 40·73	39·6	+ 1·1
γ Aquilæ	19 39 36·22	36·13	+ 0·09	79 43 31·05	30·7	+ 0·4
α Aquilæ	19 43 57·12	57·08	+ 0·04	81 29 55·32	54·5	+ 0·8
β Aquilæ	19 48 26·14	26·19	− 0·05	83 56 24·92	25·1	− 0·2
α¹ Capricorni	20 9 53·12	53·16	− 0·04	102 56 16·17	18·7	− 2·5
α² Capricorni	20 10 17·03	17·08	− 0·05	102 58 33·07	35·2	− 2·1

	Greenwich Mean Right Ascension.	Rad.	G.—R.	Mean N.P.D.	Rad.	G.—R.
	h m s	′	′	° ′ ″	″	″
α Cygni	20 36 39·60	39·47	−0·13	45 13 6·23	7·3	−1·1
α Cephei	21 15 14·13	14·18	−0·05	28 0 24·59	24·8	−0·2
α Aquarii	21 58 35·50	35·49	+0·01	90 59 54·48	55·1	−0·6
α Piscis Australis	22 49 54·38	54·40	−0·02	120 21 48·31	47·2	+1·1
α Pegasi	22 57 47·33	47·33	0·00	75 32 50·49	50·7	−0·2
α Ursæ Minoris	1 8 3·05	2·37	+0·68	1 26 12·90	13·1	−0·2
β Ursæ Minoris	18 17 30·19	30·60	−0·41	3 23 53·19	52·6	+0·6

The agreement, on the whole, between the Greenwich places and those of the *Tabulæ Reductionum* appears rather closer than with the *Radcliffe Observations*.

On the Resolvability of Star-Groups, regarded as a Test of Distance. By Richard A. Proctor, B.A.

There are considerations connected with the resolvability of star-groups which have not hitherto received much attention, so far as I am aware. They bear somewhat importantly on the opinions we are to form respecting the distribution of matter throughout the sidereal system.

In the first place, the resolvability of such clustering aggregations of stars as obviously form part of the sidereal system has been regarded as an important means of estimating the relative extension of different parts of that system. So long as a portion of such a clustering aggregation remains unresolved it has been assumed that the limits of the system in that direction lie beyond the range of the telescope which thus fails to effect resolution, and therefore that the extension of the system in that direction is far greater than in other directions where the same telescope shows the stars projected discretely on a perfectly black background.

In the second place, in the case of definite groups of stars, which either lie beyond the limits of the sidereal system, or if within those limits are yet separated from other parts of the system, and surrounded on all sides by relatively barren regions, it has been commonly assumed that we have, in the telescopic powers necessary to effect resolution, a means of forming a general estimate of the distances at which such groups may lie.

It is my purpose here to indicate certain considerations which point to opposite conclusions as respects both these cases.

If we were to accept the conclusion that where a portion of the galaxy is not resolvable with powerful telescopic aid, the sidereal system has a relatively great extension, it would follow from the smallness of the areas which many of these portions of the galaxy present that there is an extension of the system in those directions into long spike-shaped projections, lying in a di-

rection pointing exactly towards the solar system. When Sir William Herschel, for example, speaks of a region of this sort, of limited extent, which his great 40-feet reflector was unable to resolve, we must accept the conclusion that there is one of these spike-shaped projections extending (according to Sir William Herschel's own estimate) no less than 2300 times further into space than a line drawn from the Sun to *Sirius*. It is not only contrary to every law of probability that this is the real state of the case; but even if we could suppose that in this and in other similar instances such spike-shaped projections could *by mere accident* be directed along lines extending radially from the Sun, that is, if we could get over the argument from probability, there would still remain mechanical objections to our believing in such an arrangement. Knowing as we do that all the stars are in motion under the influence of their mutual attractions, and apparently also under the influence of some other and far greater forces adequate to generate the enormous observed motions, we ought scarcely to be willing to recognise in any part of the system a law of distribution which could not result from any conceivable dynamical processes.

It seems more reasonable to conclude that, where a cluster presents the peculiarity considered, there is not enormous longitudinal extension, but a real peculiarity of constitution; that, in fact, the observer has not been penetrating further and further into space as he increased his telescopic power, but simply analysing more and more searchingly a definite region of space.

In fact, Sir William Herschel, in one of his later papers, was led to consider this as perhaps the true explanation of the matter; for in 1817 we find him saying that his star guagings have in reality more direct reference to the condensation than to the distance of the stars, so that a greater number of stars in the field of view may be explained as well by a greater condensation of that portion of the galaxy, as by a greater extension of its figure in that direction in which the stars appear most numerous.*

Now as regards the case of a distinct cluster of stars, let us consider first the effect of distance on a group of stars all equal in magnitude and separated from each other by equal intervals. Supposing such a cluster so placed that the naked eye could recognise each separate orb, and then to sweep rapidly away into space, would it become nebulous or not before vanishing from view? As the group passed away each separate orb would grow less and less bright, and the distances separating orb from orb would grow apparently smaller and smaller. And clearly if these distances became too small to be distinguished, while the stars of the group yet continued visible, there would result a nebulosity of appearance. But suppose that, on the contrary, the stars of the

* It is rather surprising that in nearly all our treatises on astronomy the earlier papers by Sir Wm. Herschel receive far more attention than those he wrote when at the zenith of his fame. There is only one work I know of (Professor Grant's noble *History of Physical Astronomy*), in which Sir W. Herschel's labours are adequately represented.

group became invisible when the group was at such a distance that the intervals separating star from star would not be indistinguishable (if only the stars were brighter). Then clearly the group would vanish with increasing distance without ever becoming nebulous. Clearly also, if a telescope were employed to bring the retreating group into view, the same conclusions would hold good. A group which would become nebulous to the naked eye before vanishing would become so when examined under a telescope, let the telescope's power be what it might, while a group which would vanish without becoming nebulous to the naked eye would not become nebulous before vanishing under telescopic vision, whatever the telescopic power employed.

It is clear, then, that the nebulosity of a star group, whose members are equal and equally distributed, is a question not of distance merely but of constitution, of the relation between the size and brightness of the constituent orbs and the distances which separate them from each other.

But we may extend such considerations to the case of star-groups containing orbs of different orders of magnitude. Supposing a group of this kind to be passing away into space—as in the former case,—the question whether it would become nebulous at any stage or stages of its progress would depend on the question whether or not the order of stars about to disappear individually were congregated so closely that the eye could not distinguish the distances separating them. Clearly also it might be possible that an order of stars *not* about to disappear might present a nebulous appearance, in which case obviously all lower orders still remaining visible would be involved in that nebulous light. Such a cluster, in passing away from the eye, might also be nebulous at a certain distance, and become non-nebulous at a greater distance; all that would be necessary for such a result being that, while some of the lower orders of stars were distributed richly enough to present a nebulous appearance before vanishing, some of the higher orders should be so sparsely distributed as not to present a nebulous appearance before vanishing, or at any rate for some time after the lower orders *had* vanished.

It is further obvious that the same would be true if the retreating group were watched with a telescope of any power whatever (setting aside all question of the extinction of light in passing through space). The same appearances would be presented in precisely the same order when the group passed (star-order by star-order) out of the range of view of any telescope as when it passed out of the range of the unaided vision.

It follows that, if we apply telescopic power to a given group of stars, we can by no means conclude from the nebulosity of the group under such and such power that the group lies at such and such a distance, unless we are prepared to believe in the existence of certain laws of constitution to which all stellar clusters are subject. But such a belief is not likely to find acceptance with those who are acquainted with the observed variety in the constitution of star-groups.

It happens also that we have direct evidence that irresolvable nebulosity affords no proof of relatively enormous distance. When Sir John Herschel was surveying the neighbourhood of the lesser Magellanic Clouds he found that near the edge the Nubecula Minor was irresolvable with the 18-inch reflector, whereas the heart of this Nubecula could be clearly resolved.* Now it needs no proof that, if the Nubecula Minor (setting aside the nebulæ existing within it) were constituted of stars according to the generally uniform laws assigned to the constitution of the sidereal system, the centre of the Nubecula would be the part whose resolution would be most difficult. It is evident, therefore, that the outer parts of the Nubecula are constituted differently from the central region, and the possibility is suggested that the smaller stars seen in the central region belong in reality to the outer shell, whose real character is indicated by the irresolvability of the outer parts of the Nubecula's *disc* (as distinguished from the Nubecula's substance).

In this instance, then, it is distinctly proved that the irresolvability of a celestial region under Sir John Herschel's 20-feet reflector is no proof of relatively enormous distance. But what is thus proved for a certain telescopic power must be true of all telescopic powers. Hence, whatever the power may be under which a certain region appears nebulous, we have no proof that the stars contained within that region are further off than stars within a region resolvable under that power. But since this must be true of all powers, it must be true of naked-eye vision. Hence the stars forming the galaxy are not necessarily further off than those star-groups which the eye can resolve.

One important conclusion which is, I think, fairly deducible from what has been shown, is that, supposing a spiral of small stars such as I have suggested that the milky way may be, should extend along a part of its length, so far from the eye as to become invisible through distance, we ought not to expect that in passing from the visible part to this invisible portion all orders of resolvability down to utter irresolvability in the most powerful telescopes

* I quote the following passages from Sir John Herschel's *Notes on the Nubecula Minor*. They are all I can find which bear on the question of resolvability.

"The edge of the 'smaller cloud' comes on as a mere nebula."

"In the edge of the cloud vision bad, &c. . . . the cloud is not resolved, and seems a very mysterious object."

"We are now in *the cloud*. The field begins to be full of a faint light perfectly irresolvable."

"I should consider about this place the body of *the cloud*, which is here fairly resolved into excessively minute stars, which, however, are certainly seen with the left eye."

"Re-examined by the side motion the whole cloud, in general and in detail. The main body *is* resolved, but barely. I see the stars with the left eye. It is not like the *stippled* ground of the sky. The borders fade away quite insensibly, and are less or not at all resolved. The body of the cloud does not congregate *much* into knots, and altogether it is in no way a striking object apart from the nebula and clusters."

"Upper limit, *but here it is starry, at the other limit* nebulous."

ought to be recognised. On the contrary, this part of the spiral might exhibit in succession all the orders of change which the retreating group considered above was shown to be capable of showing (on a certain, not improbable, assumption as to its structure).

But my object is not so much to find evidence in favour of a special theory about a certain portion of the sidereal system as to indicate the varieties of appearance which are to be looked for in different parts of that system,—varieties which are, in fact, as likely to be met with (according to my views of the nature of that system) around the poles of the galaxy as in the richest portions of that wonderfully complex zone.

On an early Telescope made by Giuseppe Campani of Rome.
By John Williams, Assistant Secretary.

At the sale of the late Dr. Lee's instruments, a few weeks since, I purchased an Italian telescope, which, appearing to be of considerable interest as an example of an early instrument by a then eminent maker, I trust I may be excused calling the attention of the Meeting for a few minutes to it.

On examination I found it to be one constructed by the celebrated Joseph Campani of Rome, who was considered as the best maker of telescopes of his day. Thus we find Cassini and other eminent Astronomers of that time employing instruments made by Campani in their astronomical researches.

I have been unable to find any satisfactory biographical account of this able artist, and can only ascertain that he flourished about the middle of the seventeenth century.

Weidler, in his *Historia Astronomiæ*, 4to, 1741, speaks very highly of Joseph Campani, and particularly notices his observations of *Saturn* and *Jupiter*, as well as his excellence as a maker of telescopes. As the quotation from Weidler is rather long and is in Latin, I shall content myself with the summary of his statement by Dr. Long, as given in his *Treatise on Astronomy*, 4to, 1785. His words are "About this time also (1641) Joseph Campani at Rome applied himself to the grinding of glasses for long telescopes, which far excelled all others of that time. Through the munificence of Louis XIV. Cassini had some made by this artist of 100 and 200 palms. Campani saw the shadow of *Saturn's* ring upon his body, as also his zones or obscure belts, and detected the shadows of *Jupiter's* satellites in passing over his body. It was with one of Campani's telescopes that Cassini first saw all the satellites of *Saturn*." Weidler adds, "Longiora quidem telescopia Campanus pauca fabricavit, in quibus, pro coloribus arcendis, tria vitra ocularia adhibuisse dicitur; attamen etiam breviora quæ composuit, 15, 20, 30 pedes longa, singulari perfectione prædita erant, ut ceteris similibus antecellerent." Which may be rendered, "Campani also made a few longer telescopes, in which to neutra-

lise colour, he is said to have applied three glasses in the eye-piece; and the shorter telescopes made by him of 15, 20, and 30 feet in length, were of such extraordinary perfection that they greatly surpassed all others of the same kind." It is also stated that "the Royal Academy (of France) are said to have had excellent telescope glasses of this artist's making which were of 200 and 300 feet focus and magnified four or five hundred times." In the *Speculum Hartwellianum* Admiral Smyth mentions this very instrument as "an old one deserving notice for its respectable age and its tolerable performance."

The telescope to which your attention is now called is one of the shorter ones fabricated by this excellent maker, it being but from 9 to 10 feet in length. The object-glass is 2 inches in diameter, reduced by a diaphragm to 1½ inch, and is ¼ inch in thickness, on it is written with a diamond "Giuseppe Campani in Roma," and the glass of which it is composed is of excellent quality. The eye-piece consists of three plano-convex lenses, the glass of which is of equally good quality, that nearest the eye being fixed and the other two reversible. There are inscriptions in Italian relating to these glasses, one of which reads as follows—"Quando questa parte sta dentro al cannello si vede l'oggetto piu chiaro," *i.e.* when this part is placed within the tube the object is seen clearer. The second is in the same words, excepting that "piu grande" occurs instead of "piu chiaro," implying that the power is increased. One of the Fellows of the Society (Mr. Buckingham), who is well acquainted with telescopes, has tested these powers, and estimates the lower one at about 20 and the higher at 25 times. Upon taking this portion of the eye-piece away altogether, the telescope becomes an astronomical one, and is much increased both in power and clearness. There is also on the eye-piece a nearly obliterated inscription evidently relating to its use for astronomical purposes, together with the letters PALI. XII., which appear to refer to the focal length as being 12 Palms. The Roman Palm is about 9 inches. This would give 9 feet as the focal length, which closely approximates to the truth. The case is composed of nine tubes formed of vellum, which are remarkably strong and light, the whole weight being but 2 lbs. 11 ozs. These tubes draw out in a manner similar to that employed in telescopes of the present day. When closed it is about 2 feet in length, but draws out to about 9 feet when in focus. The workmanship of the whole is exceedingly good.

When it came into my hands it had evidently not been drawn out for many years, and I had great difficulty in extending it to its full length. The glasses also were encrusted with the dirt of years, and required careful cleaning.

In the absence of a better test, I turned it on the word "Admiralty" on the opposite side of the quadrangle of this building (Somerset House), and was very favourably impressed with the sharpness of its definition and the flatness of the field. I have since viewed *the Moon* through it, and, considering the low power,

was much gratified, and indeed surprised, at the excellence
performance; the lunar craters appearing, with all the po
sharp and well defined, and the ragged edge very beautifully sl
The field of view is, however, small, and the instrument i
troublesome to use on account of its length, as, unless very
fully handled and adjusted, the flexure occasioned by the wi
suitable support interferes somewhat with its efficiency.
the requisite care, however, it acts very fairly, and I ha
doubt, if properly mounted, its performance would be
satisfactory.

Instrument for Sale.

A Transit Instrument, $3\frac{1}{2}$ feet focal length; object-
$2\frac{1}{4}$ inches aperture; brass Y's with agate bearings for fixi
stone piers; 3 eye-pieces; micrometer; level; clamp lamp
but wanting the setting circle. Made expressly for a de
Fellow of the Astronomical Society. Apply to Mr. Wil
Assistant-Secretary, Somerset House.

ERRATUM.

Page 152, line 7 from the bottom, "needle line" should be needle *As*

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[illegible]riments of the Society, June 8, 1870.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *June* 10, 1870. No. 8.

WILLIAM LASSELL, Esq., President, in the Chair.

John Dickinson, Esq., 11 Upper Grosvenor Street, and
Capt. David Smith, Warkworth Terrace, Commercial Road,
were balloted for and duly elected Fellows of the Society.

On the Orbit of α Centauri. By E. B. Powell, Esq.

In the notes to my Second Catalogue of Binary Stars published in vol. xxii. of the Society's *Memoirs*, I gave a revised orbit for *α Centauri*, representing the results of observations up to 1862.3; and in a communication dated March 9, 1864, and published in the *Monthly Notice* for May of that year, I invited attention to the important part of the orbit about to be described, viz., the extremity of the perspective ellipse corresponding to the lesser maximum of distance. I have now to state that the comparison has, so to speak, doubled the above extremity, and that consequently the orbit can be determined very approximately. Hitherto it was impossible, as the lamented Captain Jacobs remarked, to say how far the apparent ellipse extended in a northerly direction, and correspondence between observation and calculation did not suffice to establish the correctness of a set of elements. Now, however, though even four or five additional years will enable us to improve the orbit, especially as to the time of periastral passage, the results I have arrived at undoubtedly approximate *pretty closely* to the truth. These results are as follows:—

Perspective Orbit.

Semi-axis major	17″
Semi-axis minor	2″·8
Greater maximum distance	23″·8
Position angle for ditto	210° 40′
Lesser maximum distance	10″·4
Position angle for ditto	18° 45′
Greater minimum distance	3″·98
Position angle for ditto	301° 45′
Lesser minimum distance	1″·16
Position angle for ditto	115° 30′

Real Orbit.

π	38° 40′
e	·63944
Ω	24° 18′
γ	81° 13′
a	20″·13
P	76·25 years
τ	1874·2

Without going into the details of my late observations, which will find place in a paper I shall do myself the pleasure of laying before the Society hereafter, I may mention that the position of *α Centauri* in the beginning of the year was as given below:—

Angle.	Distance.	Epoch.
20° 27′	10″·24	1870·1

The angle is the mean of 100 observations on thirteen nights; the distance, that of 162 observations on twelve nights. I feel great confidence in both measures.

I have to remark that, in the earlier orbits of *α Centauri* calculated by me, I was misled by a belief that Feuillée's observation was taken after periastral passage. Of late years it has become evident that the observation was taken before that passage. If the above mathematician's record can be fully relied on, as I imagine to be the case, it at once limits the periodic time to something less than 76·6 years. Feuillée remarks that, when he observed the star at Lima on July 4, 1709, the companion was west of the primary; but, about 1862·7, the companion crossed the meridian passing through the primary; consequently 1862·7–1709·5, or 2 (76·6) years, must be somewhat more than twice the period of *α Centauri*.

Madras,
April 20th, 1870.

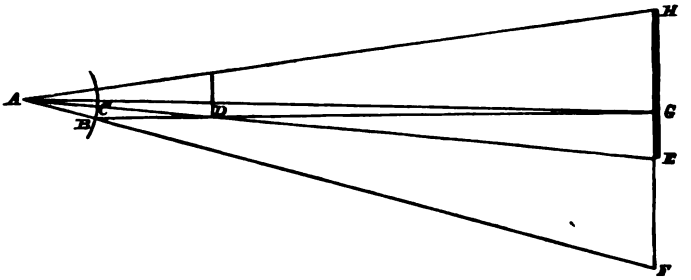
On the Determination whether the Corona is a Solar or Terrestrial Phenomenon. By George M. Seabroke, Esq.

It is my intention in this paper to attempt to show that, with the existing state of our knowledge of the corona, the theory set forth by Mr. Lockyer, that the corona is a terrestrial phenomenon, is quite possible, rather than to show that other theories are wrong,—and further to demonstrate how the question may be set at rest by observations on future eclipses. The points which present themselves are as follows:—

1. What are the facts with respect to the spectra of the corona seen in past eclipses?
2. What spectra ought we to obtain from the corona on the terrestrial theory during totality?
3. Are the spectra obtained from the corona in past eclipses reconcilable to those we ought to get on the above hypothesis?
4. What spectrum ought we to get from the corona after totality?
5. What spectrum ought we to get before totality on the following side of the Moon?
6. What difference will there be between the spectrum of the central portions of the corona and that of the distant parts during totality?

With regard to (1) during the Indian eclipse, Major Tennant writes,—"Directly I saw the whole Moon in the finder I set the cross-wires immediately outside its upper limb. By the time I got to the spectroscope the cloudy range seen in the photographs had vanished from the slit, and I saw a faint *continuous* spectrum. Thinking that want of light prevented my seeing the bright lines which I had fully expected to see in the lower strata of the corona, I opened the jaws of the slit and repeatedly adjusted by the finder, but without effect. *What I saw was undoubtedly a continuous spectrum, and I saw no bright lines.* There may have been dark lines of course, but with so faint a spectrum, and the jaws of the slit wide apart, they might escape notice." With respect to the American eclipse, Prof. Pickering, with an ordinary chemical spectroscope directed to the Sun's place during totality, saw a continuous spectrum with two or three bright lines; one "near E" and a second "near C." Prof. Young, while examining a part of the prominence at $+146°$, saw C, near D, a line at 1250 ± 20, and another at 1350 ± 20, and the 1474 K line very bright, but not equal to C and D_1; but he observed that the 1474 K line, unlike C and D_3, extended across the spectrum; and, on moving the slit away from the prominence, it persisted, while D_3 disappeared. He also believes that the two faint lines between it and D_3 behaved in like manner. On examining a prominence on the other side of the Sun, he observed nine lines and a faint continuous spectrum without any traces of dark lines in it.

As to the second point, let us find what spectrum we ought to obtain from a corona at a point on the Earth where the limbs of the Sun and Moon are in line; that is, where the eclipse is total, exactly.



Let A be a point on the Earth where Sun is eclipsed;
B C, limits of Earth's atmosphere;
D, the Moon;
H E, Photosphere of Sun;
E F, the apparent Corona.

Now, if the corona be terrestrial, the light producing it must be reflected or separated from the atmosphere within the triangle A B C.

Join B D and produce to G.

Then G is the most distant point from the limb on the Sun's disk, from which light is reflected to A by the atmosphere; and if the triangle E A F or angular extent of the corona from the Sun is given, we can find $\angle$ E A G.

The angles being small, $\frac{\angle EAG}{\angle EAF} = \frac{GE}{EF}$, approximately.

$$GE : CB :: ED : DC, \text{ therefore } GE = CB\,\frac{ED}{DC}, \qquad (1)$$

$$\text{and} \quad EF : CB :: EA : CA, \text{ therefore } EF = CB\,\frac{EA}{CA}, \qquad (2)$$

and E D = E A − A D, and A D being small in proportion to E A, E D may without great error be taken as equal to E A.

Dividing (1) by (2),

$$\frac{GE}{EF} = \frac{\frac{ED \text{ or } EA}{DE}}{\frac{EA}{CA}} = \frac{CA}{DC} = \frac{\text{Height of atmosphere}}{\text{Dist. of Moon} - \text{height of atmosphere}},$$

$$\therefore \frac{\angle EAG}{\angle EAF} = \frac{\text{Height of atmosphere}}{\text{Dist. of Moon} - \text{height of atmosphere}},$$

$$\text{EAG} = \text{EAF}\ \frac{\text{Height of atmosphere}}{\text{Dist. of Moon} - \text{height of atmosphere}}.$$

If, for example, we now take

$$\text{EAF} = 30',$$

and Height of atmosphere = 100 miles,

and Dist. of Moon — ht. of atmosphere = 240,000 miles,

then $$\angle\ \text{EAG} = 30' \frac{100}{240{,}000} = 0''{\cdot}75.$$

Therefore, the only part of the photosphere available in this case for illuminating the atmosphere is a ring of photosphere $0''{\cdot}75$ in width, and from the figure it will be seen that only that part of the corona most distant from the centre (as at B) will receive even the whole of this light; and it is manifest from the figure that the nearer any part of the corona is to the centre (nearer C) the less light will it receive from the photosphere, so that the mean illumination of the corona by the photosphere is only equal to that which would be given by a ring $\frac{1}{2} \times 0''{\cdot}75 = 0''{\cdot}375$ wide.

Now, since the chromosphere extends from E towards F, the whole of the atmosphere producing the corona is illuminated equally by the chromosphere, and since the mean height of the chromosphere is much more than $0''{\cdot}375$, or other height deduced from the foregoing formula, it is quite possible that the dark lines of the spectrum coming from so small an area of photosphere may be blotted out, as Mr. Lockyer observes, by the light from a greater area of chromosphere wherever the chromosphere contains the proper substances; and it is probable that the vapours of a number of substances from the photosphere are carried up into the chromosphere in small quantities sufficient to obliterate the dark lines, since we find the vapours of magnesium, sodium, barium, and iron, sometimes in the chromosphere.

Although the total amount of light of all kinds given by an equal area of chromosphere is small compared to that given by an equal area of photosphere, still each particular kind of light from the chromosphere is as intense, or nearly so, as that particular kind of light from the photosphere, so that, if equal areas of chromosphere and photosphere be illuminating a part of our atmosphere, that part would give a spectrum, having its dark lines erased by the chromosphere, or a continuous spectrum. When the area of the photosphere is much less than that of the chromosphere, the bright lines given by the chromosphere would be much more visible than the remaining dark lines of the photospheric spectrum.

From this it appears that during totality we ought to get from the corona a nearly continuous spectrum, with bright lines given by the substances in the chromosphere. Some of the dark lines

of the photospheric spectrum ought to remain where the chromosphere does not contain substances giving bright lines in their place. Where the illuminating areas of the photosphere and chromosphere are equal, which is possible where the chromosphere is unusually low, we ought to obtain a spectrum as above, but without bright lines, the chromospheric lines being then only just able to obliterate the dark ones.

3. In the Indian eclipse Major Tennant saw a continuous spectrum without bright lines, which is that we should obtain on the above hypothesis, when the areas of chromosphere and photosphere illuminating our atmosphere are equal; but it is shown above that during totality with the ordinary height of chromosphere the illuminating area of chromosphere is much greater than that of the photosphere, so that the part of the chromosphere illuminating that part of the corona under examination must have been unusually low, or, as was probably the case, there were bright lines, for, as he says the spectrum was very faint, they may have been missed. There ought on this hypothesis to have been dark lines, but Major Tennant says that with so wide a slit he might have missed them. Prof. Pickering saw a continuous spectrum with bright lines, which is what we ought to obtain when the atmosphere is illuminated by a greater area of chromosphere than photosphere, as has been shown to be the case when the chromosphere is at its normal height. The dark lines which ought to have been visible on Mr. Lockyer's theory might possibly have been too faint to be noticed, since, as stated above, the area of photosphere in this case would be small in proportion to that of the chromosphere, so that the bright lines would appear very plainly when the photospheric spectrum was too faint to render the dark lines visible. As to the 1474 K line observed by Prof. Young to extend across the spectrum beyond the other lines of the chromosphere, Mr. Lockyer observes that he often sees this line and often does not, which appears fatal to this being a real corona line, as, if so, it ought always to be visible. Prof. Young also seems, in the note to his observations, to be doubtful how far this line extended from the prominence; and it is very probable that this line is either iron or hydrogen. There seems to be no evidence that the other lines seen in the corona spectrum are not chromospheric lines.

4. With regard to this point, an inspection of the figure will show that, as the Moon passes over the Sun, more photospheric light becomes available for illuminating the corona; but so long as the available area of the photosphere is less than that of the chromosphere, the dark lines of the spectrum, due to the photosphere, will be erased by the chromospheric lines (wherever the chromosphere contains the proper substances), and as the Moon moves forward the spectrum should on this hypothesis change, and when the illuminating area of the photosphere becomes greater than the area of the chromosphere, the dark lines of the photospheric spectrum should appear. It will also be seen that the

larger the illuminating area of the photosphere becomes the smaller will be the difference between the spectrum of the interior part of the corona and that of the exterior part, since, whatever be the extent of the illuminating surface of the photosphere, the exterior parts of the corona will only receive an excess of light over that received by the interior part equal to the amount of photospheric light received by those parts during totality, or, as in the case above taken, the excess will be equal to that given by a ring of light from the photosphere 0″·75 wide (or G E in the figure), so that, when a few seconds of photosphere are visible to the observer, the difference between the spectra of the exterior and interior parts of the corona would be inappreciable.

5. What spectrum ought the corona to give before totality on the following side of the Moon? In this case, when the angular distance of the limits of the Sun and Moon is some seconds, the difference between the spectra of the exterior and interior parts of the corona is small, since no part of the atmosphere in this case will be illuminated by the photosphere, so we ought to obtain a chromospheric spectrum, together with a faint photospheric one caused by a small amount of photospheric light reflected from the photosphere by the chromosphere.

6. On the foregoing hypothesis during totality the parts of the corona nearest the centre should give a different spectrum to the more distant portions, since the portions nearer the centre receive less photospheric light than the more distant parts, and the same amount of light from the chromosphere.

In order to test the correctness of this theory, advantage may be taken of the following facts:—1st. At that period of the eclipse when the limb of the Sun and Moon are in line with the observer, there will be a difference between the central and distant parts of the corona, and this difference will decrease as the Moon passes on, whereas, by the other theory, there should be the same difference as long as the corona is visible. 2d. If the corona be terrestrial, the spectrum of any portion of it ought to be continually changing during the passage of the Moon; but, if solar, the spectrum should remain unchanged.

On the Displacement of the Bright Lines in the Spectrum of the Solar Chromosphere. By G. M. Seabroke, Esq.

The author in reference to a letter of Father Secchi to the Academy of Sciences of 25th April, writes,—

"For some time past, Mr. Lockyer has been kind enough to allow me the use of his telescope and spectroscope to prepare myself for the observations I intend making during the ensuing eclipse; and as I have been using the same spectroscope with which the discoveries in question have been made, and which have been contested by Father Secchi, I think it right to add my independent testimony on that point."

He remarks that the displacement of the bright lines of the spectrum of the chromosphere cannot be explained in the manner attempted by Father Secchi, by the rotation of the Sun, whatever the velocity of rotation is assumed to be. "For I have frequently seen a change of wave length in the same direction in the spectrum of prominences on opposite sides of the Sun, and if the change was produced by the Sun's rotation the change must be in opposite directions, since one side is approaching and the other receding from us. I also frequently see a change of wave length in the spectrum of one part of a prominence and not in another. How does Father Secchi's theory account for this? Besides, on his hypothesis, the bright lines of the prominences should never appear curved as I often see them, but should remain perfectly straight. The black lines should, on his hypothesis, be also displaced like the bright ones, so that the bright lines would still retain their position with respect to their corresponding black line, whatever be the velocity. Perhaps every one in eight of the prominences I have seen,—and I see two or three every time I looked at the Sun,—have given decided changes of wave length; in fact, the occurrence is so frequent, that unless any extraordinary change is seen, I make no note of it, and the changes of wave length are continually varying, and seldom last more than a quarter to half an hour, which clearly shows that the Sun's rotation has nothing to do with. That there are tremendous movements in the chromosphere is certain, from (1) the alteration of wave length observed in the space of a few minutes or sometimes seconds; (2) when a prominence is observed with a wide slit a change of form can generally be detected in a few minutes;" and he annexes rough sketches of the F line of two prominences showing change of wave length.

London,
June 1, 1860.

On a Spectroscope in which the Prisms are automatically adjusted to the minimum angle of deviation for the particular Ray under examination. By J. Browning, Esq.

In an ordinary spectroscope the prisms are usually adjusted to the minimum angle of deviation for the most luminous rays in the spectrum,—by preference I adjust them myself for the ray E in the solar spectrum. This being done, the prisms are screwed, or otherwise firmly clamped, to the main plate of the spectroscope. Thus adjusted they are liable to two sources of error, one of which places the observer at a serious disadvantage. First, only the particular ray for which the prisms have been adjusted, is seen under the most favourable circumstances, for only this ray passes, as all should do, through the train of prisms parallel to the base of each prism. Of more importance than this, however, is the

fact that the last prism of the train being fixed while the telescope through which the spectrum is viewed is moveable around an arc, it is only when the central portion of the spectrum is being examined, that the whole field of the object-glass is filled. By means of the models, as well as the instrument, which is before you, I hope to make myself clear on this point.

In figure 1, P P &c., represent a train of prisms adjusted as I have just described for the central portion of the spectrum, and screwed firmly in their places. T represents a telescope moving round a centre situated at K. In the position in which

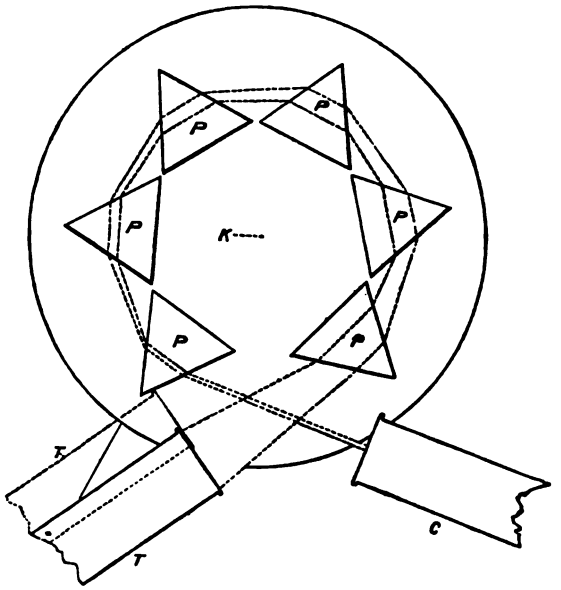


Fig. 1.

the telescope is placed, the whole field of the object-glass would be filled with the green light of the spectrum issuing from the last prism; but when the telescope is removed to the position shown by the dotted lines, either nearer to R or to V (in which case the red end or the violet end of the spectrum would be in the field of view), then, as you will see by the lines, only a small portion of the spectrum would fall on the object-glass. But it is obvious that owing to the deficiency in light at the extreme ends of the spectrum, it is just in these very positions that it is desirable that the whole field of the object-glass should be filled. Now this can only be effected when the prisms are adjusted to the minimum angle of deviation for the particular portion which is being examined of the spectrum; and this it will be if the adjustment I have spoken of has been correctly made. This

difference in adjustment is much more than is generally supposed, varying, in accordance with the refractive index of the glass employed, between 10° and 20° for the extreme portions of the spectrum.

Bunsen and Kirchoff, when making their celebrated map of the solar spectrum, adjusted the prisms they used (four in number) for the principal Fraunhofer lines; but the trouble of doing this is so great that few observers have ever seen the extreme portions of the solar spectrum under favourable circumstances.

A distinguished professor of natural philosophy informs me that he once adjusted a train of prisms for the line H in the solar spectrum, but that he found the experiment so troublesome that he should not be likely to repeat it unless for purposes of accurate investigation it were indispensable.

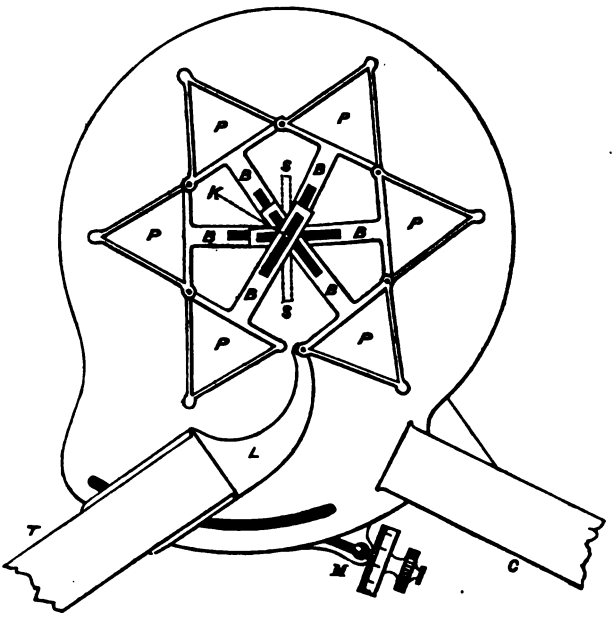


Fig. 2.

Diagram 2 shows the method in which the change in the adjustment of the prisms to the minimum angle of deviation for each particular ray is made automatically. In this diagram, P P &c., as before, represent prisms. All these prisms, with the exception of the first, are unattached to the plate on which they stand. The triangular stands on which the prisms rest are hinged together at the angles corresponding to those at the bases of the prisms. To each of these bases is attached a bar, B, perpendicular to the base of the prism. As all these bars are slotted and run on a common centre, the prisms are brought into

a circle. This central pivot is attached to a dovetailed piece of two or three inches in length, placed on the under side of the main plate of the spectroscope, which is slotted to allow it to pass through. On moving the central pivot the whole of the prisms are moved, each to a different amount in proportion to its distance in the train from the first or fixed prism, on which the light from the slit falls after passing through the collimator, C. Thus, supposing the first prism of the train of C, represented in the diagram, to be stationary, and the second prism to have been moved through 1° by this arrangement; then the third prism will have been moved through 2°, the fourth through 3°, the fifth through 4°, and the sixth through 5°.

Now for the contrivance by which this arrangement is made automatic. A lever, L, is attached by a hinge to the corner of the triangular plate of the last prism. This lever, by its further end, is attached to the support which carries the telescope, through which the spectrum is observed. Both the telescope and lever are driven by the micrometer-screw, M. The action of the lever is so adjusted that, when the telescope is moved through any angle it causes the last prism to turn through double that angle. The rays which issue from the centre of the last prism are thus made to fall perpendicularly upon the centre of the object-glass of the telescope, T, and thus the ray of light travels parallel to the bases of the several prisms, and ultimately along the optical axis of the telescope itself, and thereby the whole field of the object-glass is filled with light.

Thus the apparatus is so arranged that on turning the micrometer-screw, so as to make a line in the spectrum coincide with the cross-wires in the eye-piece of the telescope, the lever L, attached to the telescope and prisms, sets the whole of the prisms in motion, and adjusts them to the minimum angle of deviation for that portion of the spectrum.

Fig. 3.

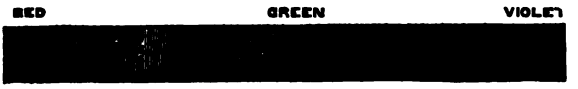




Fig. 4.

Diagrams 3 and 4 represent the appearances presented when looking through the telescope from which the glasses have been removed. In diagram 3 it will be seen that the whole circle of the object-glass is filled with light, as I have just described, is the case with the new arrangement; while diagram 4 shows the

effect of moving the telescope through the angle in front of the fixed prism.

About three years ago I showed a rough model of the plan I have now described, to Mr. Gassiot, for whom I made the large instrument which was some time in use at the Kew Observatory. Mr. Gassiot immediately asked me if I would apply this arrangement to his large spectroscope. As I did not at that time see my way to make it self-acting by connecting the prisms with the micrometer-screw, I did not feel impelled to carry out the matter at once, owing to the fact that about this time the mapping of the solar spectrum with the large spectroscope in question was discontinued. I also felt that, with that munificent liberality for which Mr. Gassiot has distinguished himself, he had simply asked me to add this contrivance to his large and costly instrument in the interest of science generally, and not with any view to its immediate use.* Since this time, and particularly while I was constructing his solar spectroscope, Mr. Norman Lockyer has repeatedly urged upon me the desirability of completing the arrangement; and from the manner in which it has been received by the distinguished scientific men who have done me the honour to examine it minutely, I am induced to hope that its application will tend to facilitate further researches in spectrum analysis.

Note on the Alteration in the Colour of the Belts of Jupiter.

By John Browning, Esq.

In the Report of the Astronomer Royal to the Board of Visitors, there is the following remark:—

"There has been little opportunity of employing the instrument on other objects, except in a drawing of *Jupiter* by Mr. Carpenter.

"The comparison of this with drawings made eight or nine years ago appears to negative the idea of any change in the colour of *Jupiter's* belts."

With all possible deference to the Astronomer Royal and Mr. Carpenter, I beg to submit that a comparison of drawings made either this year or last year with others made eight or nine years ago, might not throw any light on this question. There is, as I have pointed out in my previous papers on this subject, some reason to believe that the change in colour in the equatorial belt of *Jupiter* is periodical. This belt of the planet at the time mentioned may have been of the same colour as during the last presentation,—that it was differently coloured during the presentation of 1868–9, is a fact attested by some six or seven, at least, well-known and skilful observers. It is true

* I am indebted to M. Gassiot for the opportunity of exhibiting the instrument on this occasion.

that in nearly every case these observers were using *reflectors*, of apertures varying between 6 and 12-inch, but I have heard from observers who have used achromatics varying between 4 and 8-inch, that they distinctly remarked the change in colour, although they do not seem to have seen it so plainly as those who have used reflectors. Unfortunately, none of these observers have published the results of their observations.

There is a singular agreement between observers who have described this phenomenon. The colour has been described as yellow, ochreish, brown, and tawny. All agree in ascribing this colour to the equatorial belt of the planet, ordinarily seen of a pure, or at most a pearly white.

The change has been one that might have been easily observed even by those who have not a quick eye for colour, by reason of the fact that the equatorial belt, which is ordinarily the brightest portion of the disk of the planet, and is so represented in every drawing I have ever seen of it, was, during the last presentation, much darker in colour than the bright belts nearer to the north and south poles of the planet.

Clapham, June 10th.

On the Proper Motion of Groombridge, 1830.

By W. T. Lynn, B.A.

In the year 1842 Professor Argelander announced* that he had detected that a star of the seventh magnitude on the boundaries of the constellations *Ursa Major* and *Canis Venatica* was animated by a Proper Motion considerably exceeding that known for any other star; amounting, in fact, annually to about 7″ of a great circle. This star had been observed by Groombridge, being numbered 1830 in his Catalogue of Circumpolar Stars, which was reduced to the epoch January 1, 1810, and edited by the Astronomer Royal, the date of publication being 1838. Having ascertained from Mr. Airy the exact times of Groombridge's observations, Argelander was able, the year after his discovery, to state† that the star's annual Proper Motion amounted to +5″·167 in R.A. and +5″·70 in N.P.D. This satisfied within the limits of probable error of observation all the other positions observed by Lalande, Bessel, Argelander, and Nicolai.

It occurred to me recently that, as a large number of very accurate observations of this star had been made at the Royal Observatory, Greenwich, since the year 1843, in which Professor Argelander made this determination, and that these observations extended now over more than a quarter of a century, it might be worth while to put them together, with the view of ascertaining whether the large Proper Motion in question continues uniform.

* *Astronomische Nachrichten*, No. 455. † Ibid. No. 475.

This comparison I now offer to the Society; remarking that, by the kind permission of the Astronomer Royal, I am enabled to include the result of the whole of the positions contained in the New Seven-year Catalogue of 2760 Stars, which is now passing through the press, and also that derived from the observations made last year, which are still in manuscript. In bringing back the observations to the 1st of January of the year which forms the epoch of each catalogue, the proportional part of the proper motion, as given by Argelander and copied into the British Association Catalogue, has been carefully applied. This had not indeed been done in the 12-year and 6-year Catalogues; but at the end of the 7-year Catalogue for 1860, published as an Appendix to the *Greenwich Observations* for 1862, will be found (pages {cxxi.} and {cxxiii.}) the positions which would have been given in those catalogues, had this been done.

Positions of *Groombridge*, 1830.

Epoch.	R.A.	No. of Obs.	N.P.D.	No. of Obs.	Authority.
	h m s		° ′ ″		
1845	11 44 1·53	13	51 10 10·90	16	12-year Catalogue.
1850	11 44 19·04	17	51 12 20·04	19	6-year Catalogue.
1860	11 44 53·91	18	51 16 37·44	8	7-year Cat. (1860).
1864	11 45 7·83	8	51 18 21·22	8	7-year Cat. (1864).
1869	11 45 25·24	2	51 20 29·84	2	Gr. Obs. 1869, MSS.

From this we obtain the following mean annual variations for the years comprised between each successive epoch:—

Years.	Ann. Var. in R.A.	Ann. Var. in N.P.D.
	s	″
1845–1850	+3·502	+25·83
1850–1860	3·487	25·74
1860–1864	3·480	25·94
1864–1869	+3·482	+25·72

If we subtract from each of these the annual precession, which in R.A. is $+3^{s}\cdot144$, and in N.P.D. $+20''\cdot01$, we find finally for the annual Proper Motion in each interval the following values:—

Years.	Proper Motion in R.A.	Proper Motion in N.P.D.
	s	″
1845–1850	+0·358	+5·82
1850–1860	0·343	5·73
1860–1864	0·336	5·93
1864–1869	+0·338	+5·71

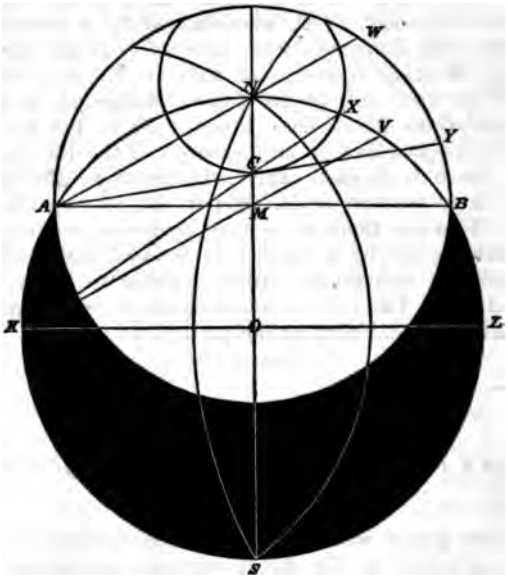
The constancy, or nearly so, of the Proper Motion of this star, would seem, therefore, to be established.

The Society have received from Mr. F. Abbott a communication, entitled "Some further Observations on the Variable Star η *Argûs* and the Surrounding Nebula," dated Hobart Town, Tasmania, 15th February, 1870, accompanied by a drawing of the nebula taken 28th January, 1870. He refers to his former drawings, 1863, *Monthly Notices*, vol. xxiv. p. v., and 1868, ditto, vol. xxviii. p. 200, and to the *Cape Monograph*, and remarks that the alterations which have taken place in the nebula since 1868 will be at once seen on an inspection of the drawings and by comparing them with each other. In the *Cape Monograph* the dark space is an inclosure. In 1863 it had two openings, one at each end. In 1868 there were four openings, and now in 1870 there are five, exposing a number of isolated and distinct stars, which render it difficult but from position to know which is the star η *Argûs*. There is attendant on these changes an increase of light which is notable up to the present time.

On a Property of the Stereographic Projection.
By Prof. Cayley.

I am not aware whether it has been noticed that the very same circles which in the direct stereographic projection of a hemisphere (viz., that wherein the projection is on the plane of a meridian) represent the meridians and parallels respectively, —represent also in the oblique projection of the hemisphere meridians and parallels respectively. In fact, in the direct projection where the poles N, S, are in the horizon-meridian, or bounding circle of the projection, if we take a chord A B at right angles to N S, and on A B as diameter describe a circle, the original (meridian and parallel) circles will, as the appearance of the figure at once suggests, represent meridians and parallels in the oblique projection in which the horizon or bounding circle of the projection is the circle diameter A B, and where consequently the North Pole N is brought into view, the South Pole S being beyond the limits of the projection. That this really is so, is clear from the consideration that in any stereographic projection whatever, the meridians will be circles passing through two fixed points N, S, and the parallels be circles cutting the meridians at right angles. (Or, what is the same thing, the parallels also pass each of them through two fixed imaginary points, the antipoints of N, S, but this in passing.) And, moreover, since in the oblique, as well as in the direct projection, the longitude of any meridian, as reckoned from the central meridian N S, is the angle at N between the two meridians, the longitude for a given meridian is the same in the two projections respectively. But the co-latitudes are not the same in the two projections respectively; viz., a circle which in the

direct projection represents the parallel co-latitude c, will in the oblique projection represent the parallel of a different co-latitude



c'. The relation between the values of c, c', will of course depend upon the position of the bounding circle A B of the oblique direction: to define this position, we may use either the arc N M which in the direct projection determines the co-latitude of the centre M of the oblique projection (say $NM = \Delta$, that is, $NV = \Delta$), or by the arc N M which in the oblique projection determines the distance of N from the centre, or co-latitude of the centre (say $NM = \Delta'$, that is, $BW = \Delta'$.) The obliquity in the oblique projection is thus $90° - \Delta'$, viz., this is the inclination of the plane of projection to that of the horizon-meridian in the direct projection. We have also $c = NX$, $c' = WY$. The relation between the angles Δ, Δ', is easily found to be

$$\tan \tfrac{1}{2}\Delta = \tan^2 \tfrac{1}{2}\Delta',$$

viz., taking the radius in the direct projection to be $= 1$, we have

$$OM = \tan \tfrac{1}{2}(90° - \Delta),$$

$$MA = \sqrt{1 - \tan^2 \tfrac{1}{2}(90° - \Delta)},$$

$$MN = 1 - \tan \tfrac{1}{2}(90° - \Delta);$$

wherefore

$$\sqrt{1 - \tan^2 \tfrac{1}{2}(90° - \Delta)} \cdot \tan \tfrac{1}{2}\Delta' = 1 - \tan \tfrac{1}{2}(90° - \Delta).$$

and thence

$$\tan^2 \tfrac{1}{2}\Delta' = \frac{1 - \tan \tfrac{1}{2}(90^\circ - \Delta)}{1 + \tan \tfrac{1}{2}(90^\circ - \Delta)} = \tan \tfrac{1}{2}\Delta,$$

the required relation.

We have moreover

$$\begin{aligned} \mathrm{N\,C} = 1 - \tan \tfrac{1}{2}(90^\circ - c) &= \mathrm{A\,M}\left\{\tan \tfrac{1}{2}\Delta' - \tan \tfrac{1}{2}(\Delta' - c')\right\} \\ &= \sin \Delta' \left\{\tan \tfrac{1}{2}\Delta' - \tan \tfrac{1}{2}(\Delta' - c')\right\} \\ &= 2 \sin^2 \tfrac{1}{2}\Delta' - \sin \Delta' \tan \tfrac{1}{2}(\Delta' - c'), \end{aligned}$$

that is

$$\tan \tfrac{1}{2}(90^\circ - c) = \cos \Delta' + \sin \Delta' \tan \tfrac{1}{2}(\Delta' - c')$$

$$= \frac{\cos \tfrac{1}{2}(\Delta' + c')}{\cos \tfrac{1}{2}(\Delta' - c')},$$

or, what is the same thing,

$$\frac{1 - \tan \tfrac{1}{2}c}{1 + \tan \tfrac{1}{2}c} = \frac{1 + \tan \tfrac{1}{2}c' \tan \tfrac{1}{2}\Delta'}{1 + \tan \tfrac{1}{2}c' \tan \tfrac{1}{2}\Delta'},$$

that is

$$\tan \tfrac{1}{2}c = \tan \tfrac{1}{2}\Delta' \tan \tfrac{1}{2}c',$$

which is the required relation between c and c'. In the particular case $\Delta = \Delta' = 90^\circ$, the two projections coincide, and we have, as we should do, $c' = c$.

Note respecting Solar Spots visible to the Naked Eye.

By A. R. Hill, Sub. Lieut. R.N.

On Sunday, 22d May, at from about 5 to 6.30 P.M., the extreme light of the Sun being obscured by a peculiar scud drifting over it, and giving the whole disk a reddish appearance, with the borders less luminous than the centre, I observed three large spots, A, B, and C, as in diagram, fig. 1, distinctly visible with the naked eye, especially A and B, A being the most distinct, and C being only visible at times, when the ardour of the Sun's rays was diminished more than at others.

On Monday, 23d, at about the same time P.M., the atmosphere being still in the same state as on the previous day, but more advantageous for naked-eye observations, I observed the spots, A, B, and C again, C being more distinct than on the previous evening; and also another spot, D, fig. 2.; but this latter being only visible at the most advantageous intervals.

During these two days the barometer averaged about 30·20 in. and thermometer, Fahrenheit, 66° in the shade at the times of observation.

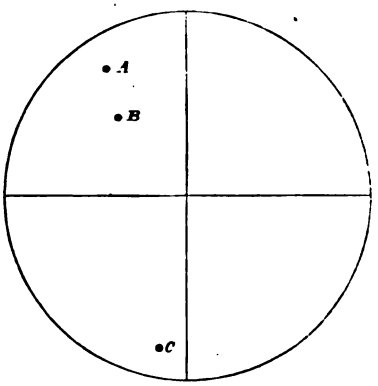


Fig. 1.

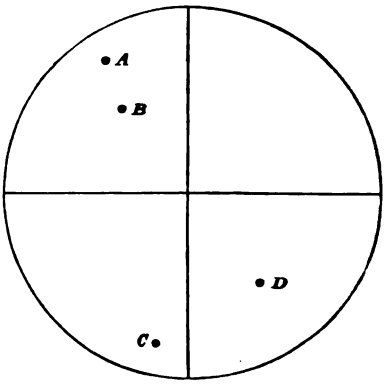


Fig. 2.

Norton, Presteign, Radnorshire, May 30th, 1870.

Winnecke's New Comet.

(*Extract of a Letter from Dr. Winnecke to Mr. Hind, dated Carlsruhe, May 31.*)

"You have probably received through the Vienna Academy the news of a small comet discovered by me in the night, May 29–30, in *Pisces*.

"I can send you to-day the following observations:

			Comet – Star.	
May 29	$14^h\ 12^m\ 38^s$ M.T. Carlsruhe	$\Delta\alpha = +0^m\ 13^s{\cdot}55$		8 Comp.
	14 13 22 ,,	$\Delta\delta = +0'\cdot\ 9''{\cdot}9$		5 Comp.

The star of comparison is only to be found in the *Bonner Durchmg.* 9.3, 1855·0.

$$\alpha = 0^h\ 47^m\ 55^s{\cdot}9 \qquad \delta = +29°\ 1''{\cdot}5$$

The last night was very cloudy, so that I have got but 3 comp. with a star twice observed by Argelander, *Bonn. Beob.* VI. + 28° No. 159.

M.T. Carls.	Comet's R.A.	Comet's Decl.
May 30 $14^h\ 13^m\ 34^s$	$= 0^h\ 50^m\ 9^s{\cdot}55$	$= +28°\ 52'\ 18''$

The comet is a round, pretty bright nebula, of about 2½ minutes in diameter"

Elements and Ephemeris of Winnecke's New Comet.

(*Communicated in a Letter from Dr. Winnecke to Mr. Hind.*)

The elements, which depend upon observations at Carlsruhe to June 2, and one made by Prof. Argelander on June 5, are as follow :—

T = 1870, July 12·905, M.T. at Berlin.

π = 302 14 53
Ω = 140 3 45 } Apparent Equin. June 2.
i = 57 19 17
log q = 9·99579

Retrograde.

Ephemeris for 12^h, Berlin.

1870.	R.A.	δ	Log r.	Log Δ.
	° ′	° ′		
May 30	12 30·7	+28 53·8	0·0910	0·2078
June 3	13 42·7	27 51·0	0·0775	0·1779
7	15 0·6	26 37·2	0·0643	0·1445
11	16 26·9	25 8·9	0·0517	0·1070
15	18 4·4	23 21·0	0·0398	0·0649
19	19 58·1	21 6·6	0·0289	0·0173
23	22 14·9	18 14·4	0·0192	9·9632
27	25 5·3	14 27·2	0·0109	9·9018
July 1	28 46·5	9 16·1	0·0042	9·8328
5	33 45·7	+ 1 59·0	9·9994	9·7571

Observations of Winnecke's New Comet. By S. J. Perry, Esq.

The comet discovered on May 30, at Carlsruhe, by Dr. Winnecke, was observed here on June 6, at about 12^h 30^m G.M.T. Its brilliancy must be considerably greater than that of the comet found last year by the same astronomer. Since it was picked up almost immediately with a low power, although the light of both Sun and Moon were sufficiently strong to illumine the field distinctly, and the object was not many degrees from the eastern horizon.

The co-ordinates corrected for instrumental errors and refraction were on June 6, at 17^h 45^m 38^s·5 sidereal time.

R.A. 0^h 57^m 3^s·6 N.P.D. 62° 49′ 31″·6 .

Light clouds prevented my obtaining a position of June 7, and last night the sky was completely obscured by clouds. I hope to observe it again when the Moon rises after midnight. I have several times swept in vain for D'Arrest's comet.

June 8, 1870,
Stonyhurst Observatory, Whalley.

Instrument for Sale.

A Dip Sector or Dipping Needle, German make, in a handsome case, quite new, never having been used, provided with a mirror, by which the deviations of the magnetic needle are indicated with the greatest possible accuracy. For further particulars apply to the Assistant-Secretary at the Rooms of the Society.

ERRATA.

Page 176, line 2 from bottom, *for* "richly" *read* "sickly."

Page 179, line 8 from bottom, *for* Feb. 2, *read* Feb. 10.

,, line 7 from bottom, *for* $9^h\ 7^m\ 14^s{\cdot}9$. G.M.T., *read* $9^h\ 8^m\ 10^s$, G.M.T.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *Supplementary Notice.* No. 9.

Total Eclipse of the Moon, 1870, *July* 12. By the Astronomer Royal.

I made no observations of the eclipse, except in regard to the intensity of illumination of the Moon's disk at the middle of the eclipse. I remarked as follows :—

1. The difference in the intensity of illumination on the northern portion of the disk (least illuminated) and the southern portion (most illuminated) was much greater than I expected to see in an eclipse in which the Moon passed so near to the centre of the shadow.

2. With my eyes unarmed, being extremely short-sighted (the distance for distinct vision less than 5 inches), and every luminous object being seen as a broad blurr, I can compare the quantity of light from sources of very different character. Thus I found that the quantity of light received from the Moon was less than that from *Saturn* and greater than that from α *Aquilæ*, including some from β and γ *Aquilæ*.

3. The light of the Moon increased considerably in ten minutes after the time of central eclipse.

4. I infer from Nos. 1 and 3 that the only part in which the shadow is nearly total is the centre of the shadow, and that a large amount of light falls within the geometrical limits of the true shadow.

July 18, 1870.

On the Lunar Eclipse, 12th July, 1870.

By C. H. Weston, Esq.

I had hoped, with an Observatory commanding from its elevated position an angle of more than 90° declination, and an uninterrupted eastern horizon belonging to a circle of 120 miles in circumference, to have well witnessed the "First contact with the Shadow" on the Moon's Eastern Limb, north of her equator, and near the Ringgebirg *Olbers*. A fixed bank of clouds, however, prevented any observation until the Moon had risen about 8°, when the Earth's shadow was found to have already covered *Hevel*, *Cavalerius*, and parts adjacent to *Reiner*, and had reached the resplendent cliffs of *Aristarchus*, and the almost equally brilliant highlands between *Aristarchus* and *Herodotus*.*

At this time the whole of the western edge of the shadow was dark, while the east periphery of the Moon was brightly visible. In 10 minutes the shadow north of the equator had reached the base of the great *Copernicus* and districts not far east of *Tycho* (including part of his radial system) on the south; then passing *Eratosthenes*, it touched the southern extremity of the mountain-range of *Apenninus* three minutes later.

After an interval of six minutes the extensive surface, including west of *Sinus Iridum* on the north, and those of the more southerly *Archimedes*, together with the greatest part of *Mare Nubium* and *Tycho* on the south, was observed in shadow.†

At this time the east periphery was reddish, and the western edge of the shadow orange, resulting from the combination of the red tinge and the yellow lunar light beneath. About six minutes later the shadow had reached the districts of the *South Pole*, and near the eastern part of *Mare Serenitatis*. Before the Moon had been so far obscured the *Sinus Iridum* was visible, but after reaching the last described limits all the shaded surface showed its details—the absence of light allowing perhaps a greater dilatation of the pupil of the eye. Now *all* the eclipsed parts were reddish, and the western margin of the shadow purplish.

After 15 minutes the shadow reached as far as the western edge of *Mare Serenitatis* and the eastern portion of *Mare Tranquillitatis*, and on the north, near *Hercules*, and not far from *Endymion*. In two minutes the eastern margin of *Mare Fecunditatis* was in-

* The telescopes used were Newtonians, 9 and 14·25 inches and 9 and 16 feet respectively — the former on the back of the latter — on the same equatoreal mounting, and thus most readily available for comparative observations.

† During the eclipse, and some days before and after full moon, the region of *Archimedes* was well seen. Besides this Ringgebirg there were two others visible, nearly as large in circumference, but apparently less in elevation, than *Archimedes*, more resembling incipient rings common on other parts of the lunar surface. These were clearly the "incipient or low protrusions, *circular and rugged*," mentioned in my paper of December last, and but indistinctly seen in the earlier phases of the Moon.

volved, and two minutes later the eastern part of *Mare Crisium* and the Ringgebirg *Geminus* further north. At this time the bright western crescentic limb of the Moon, contrasted with the eclipsed portions, afforded a fine example of the enlarging effects of irradiation. The disturbed district between *Mare Crisium*, *Mare Tranquillitatis*, and *Mare Fecunditatis*, were finely brought out just before entering the penumbra, and *Proclus* (nächst *Aristarch* das hellste Ringgebirg, B. & M.) was strikingly prominent.

After an interval of five minutes the shadow extended to the west of *Mare Fecunditatis*, and nearly west of *Mare Crisium*, and about three minutes later the phase was total.*

Now light appeared gradually to extend itself eastward over the lunar surface, and in about eight minutes after the total eclipse the converging ray-system of *Tycho* on the south, and all the country east and west of *Sinus Iridum* on the north, came out to view, while the region about the first impact was still darkish. In about 40 minutes the light was more uniformly diffused over the Moon, and after quarter of an hour the illumination was pretty equal on the east, and west, and south, while the north was darkest. After 10 minutes the east side was the brightest. Small star near the S.W. limb was well seen.

After six minutes the south-eastern periphery was brightest. *Tycho's* circular structure loomed out. The Moon gradually became darker after the mid-phase, and then the western parts also gradually less visible. *Aristarchus* was a very brilliant object.

Soon the reddish tint on the north-west appeared, which in about four minutes extended over the surface towards the south-east. Then came symptoms of approaching dawn, and two minutes later the north-east above *Grimaldi* began to brighten. Then the concave band of solar light appeared on the lunar disk, passed over *Grimaldi* and the disturbed regions east of, but distant from, *Gassendi*, and in four minutes illuminated the south-east of *Mare Humorum*. Clouds now began to cross the Moon.

Later (six minutes) the light dawned upon *Cichus* (eine grosse Vertiefung, B. & M.), and further south to the south-east of *Tycho*. More clouds. Later obtained a glimpse, and found the light had reached *Tycho*. Another 20 minutes, and the west of the north end of the range of *Apenninus* was illumined. Nothing afterwards was visible.

During the progress of the eclipse the advancing convex shadow of the Earth was observed to produce on the Moon's spherical surface a series of arcs, not *concentric* but *convergent* towards the north periphery; and it should be noted that the returning light impinged first at a point not identical with the shadow's first contact (which must have been near *Olbers*), but at a point a little

* The region between *Schubert* and the western periphery of the Moon (the last portion involved in shadow) was apparently more extensive than in Mean Moon, and doubtless arose from her libration, the maximum of which would occur two days later.

north-east of *Grimaldi*, several degrees further south than *Olbers* —visible effects of the different relative positions and motions of the Earth and Moon during the period of the eclipse.

Ensleigh Observatory,
Lansdowne, Bath, July 1870.

On Mr. Browning's Spectroscope. By M. C. de Littrow.
(*Note addressed to the Foreign Secretary.*)

I take the liberty to let you know that the essential idea in Mr. Browning's arrangement of the spectroscope (*Monthly Notices*, vol. xxx. p. 198) has already been executed and published by my deceased son Otto, in the year 1862, as you will find in the annexed treatise (*Proceedings of the Imperial Academy of Sciences of Vienna*, vol. xlvii.). The only difference between the two instruments consists in the sparing of the second telescope, which my son has effectuated by placing a mirror behind the last prism, so that the same telescope serves also as a collimator. Besides this advantage, he obtained thus the double effect of the employed prisms. He enclosed the whole apparatus in its box, so that he needed not darken the room in which he experimented.

I beg the favour of bringing this short note to the knowledge of the readers of your *Monthly Notices*.

Vienna, July 18, 1870.

Note on the Automatic Spectroscope. By John Browning.

When constructing the automatic spectroscope which I have recently had the honour of describing to the Society, I saw the desirability of giving a motion to the telescope without the use of a cam. Having spoken to Mr. Proctor on this subject, he told me that he had been convinced, while the new spectroscope was under discussion at the last meeting, that my principle really involved the complete solution of the problem of securing minimum deviation for all orders of light-waves. It was only necessary for the purpose that the slot movement should be extended to the first prism, and to the viewing telescope. It is obvious that in this way the necessary amount of motion will be communicated to the telescope, and also to the first prism, which in the former arrangement was a fixture. I am making an instrument containing this improvement, and hope to have the honour of exhibiting it at the next meeting of the Society in November.

111 *Minories, July* 2, 1870.

Note on Mr. Browning's Automatic Spectroscope.

By Richard A. Proctor, B.A.

It occurred to me, while Mr. Browning's spectroscope was under discussion at the last meeting of the Society (and more particularly when Prof. Pritchard raised the question whether all rays pass through the battery with minimum deviation for each prism), that by a slight modification Mr. Browning's ingenious device was capable of more completely fulfilling the conditions of the problem suggested by Prof. Pritchard. I am not sure that practically the spectrum will be viewed under much more favourable conditions by the plan I am about to propose; but fortunately the plan has (Mr. Browning tells me) other advantages. I need hardly say that I only present it as a modification of his most ingenious plan, and claim no separate merit for the new arrangement, even though the optical performance of the battery of prisms should be appreciably improved by it.

It is necessary to premise that although it is possible, as I shall show, to secure minimum deviation for rays of all degrees of refrangibility, yet we can obtain only an approximation to that condition which minimum deviation is intended to secure. Even in the case of a single prism, the primary and secondary foci of the emergent pencil corresponding to rays of given refrangibility are not absolutely coincident when such rays pass through the prism with minimum deviation. If φ and ψ' represent the angles of incidence and emergence, φ' and ψ the angles of first and second refraction, we have for the distance between the primary and secondary foci of the emergent pencil the expression

$$\left[\frac{\cos^2\varphi'\cos^2\psi}{\cos^2\varphi\cos^2\psi'}u + \frac{t\cos^2\psi}{\mu\cos^2\psi'}\right] - \left[u + \frac{t}{\mu}\right]$$

where u represents the distance of the origin of the source of light from the point of incidence, t the length of the path within the prism, and μ the refractive index of the prism for the rays considered. Hence, that the primary and secondary foci of the emergent pencil may coincide, we must have

$$(\cos^2\varphi'\cos^2\psi - \cos^2\varphi\cos^2\psi')\,\mu u = t\cos^2\varphi\,(\cos^2\psi' - \cos^2\psi).$$

Only when t vanishes does this reduce to the condition

$$\cos^2\varphi'\cos^2\psi = \cos^2\varphi\cos^2\psi'$$

or

$$\varphi = \psi',$$

which is the condition of minimum deviation.

Now t is ordinarily of appreciable length for each prism, in the case of a battery of prisms; and therefore the resulting distance between the primary and secondary foci is also appreciable, though small, even in the case of the first prism of the battery,

and will increase with each successive prism. The expression for this distance reduces, in the case of minimum deviation, to

$$\frac{t}{\mu}(1-\mu^2)\tan^2\psi',$$

or, if the refracting angle of the prism be $2i$, to

$$\frac{t}{\mu}(1-\mu^2)\tan^2 i. \qquad (1)$$

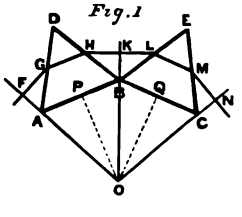
The circle of least confusion will therefore have a radius represented by

$$\frac{t\lambda\,(1-\mu^2)\tan^2 i}{\mu u\,[\sqrt{1-\mu^2\sin^2 i}+1]+t\,[(1-\mu^2\sin^2 i)^{\frac{1}{2}}+1]}$$

where λ is the breadth of the pencil at the place of émergence. It is obvious, however, that this expression will be minute under the ordinary conditions of spectroscopic observation.

Setting the condition of minimum deviation, instead of the true optical condition, as that which we are to aim at, it is obvious that in the case of a battery of prisms, all of equal refracting angle, what we require is that, first, the bases of all the prisms shall always circumscribe a circle; and, secondly, that the angle of incidence on the first surface of the first prism shall be equal to the angle of emergence from the second surface of the last prism. If these conditions are fulfilled, the condition for minimum deviation must necessarily be fulfilled; if they are not fulfilled, there cannot be minimum deviation.

Fig. 1



Suppose, for instance, that A D B and B E C (fig. 1) are two prisms of equal refracting angles, D and E, and connected, as in Browning's spectroscope, at the angle B; and let F G H L M N be the path of a ray refracted with minimum deviation through each.

Then

$$\cos \mathrm{B\,H\,K} = \mu \sin\frac{\mathrm{D}}{2} = \mu \sin\frac{\mathrm{E}}{2} = \cos \mathrm{B\,L\,H};$$

that is, B H L is an isosceles triangle, and O B K, the bisector of the angle D B E, is perpendicular to H L.

Now P O and Q O, the rectangular bisectors of A B and B C, meet on B O, as at O; and O A F makes with G A the angle G A F, equal to the angle H B K. But the angle A G F is equal to the angle B H K. Hence the angle A F G is equal to the angle B K H, or is a right angle.

In like manner O C N is at right angles to M N.

If another like prism be added at C, the same reasoning will apply; and so on, whatever be the number of prisms.

It follows that the lines from a single point O to the attached angles A, B, C, &c. of any such series of prisms are perpendicular to the course of the ray between the prisms. But any two perpendiculars O P and O Q from this point O on successive base-lines A B and B C are obviously equal; hence all such perpendiculars are equal; and therefore A B, B C, &c. are all tangents to a circle about O as centre. Further

$$\angle \text{FGA} = \angle \text{BHK} = \angle \text{BLH};$$

that is, the angle of incidence on any prism of the series is equal to the angle of incidence on the next. Therefore the angle of incidence on the first prism is equal to the angle of incidence on the last, and therefore to the angle of emergence from the last.

These are the conditions, then, which must be fulfilled in all positions of a self-adjusting battery, if rays of all degrees of refrangibility are to pass with minimum deviation through each prism, viz.

1st. *All the bases must be tangents of a circle.*

2dly. *The pencil must emerge from the last prism at the same angle at which it fell on the first prism.*

The first condition Mr. Browning's plan obviously fulfils, for the perpendiculars bisecting the equal bases all pass through one point.

The second condition the plan does not, as first devised, accurately fulfil, because the first prism is fixed, and the incident white light falls on it from the slit at a constant angle; so that there can be minimum deviation only for rays of a certain degree of refrangibility.*

The modification I suggest consists merely in fixing an angle (the angle corresponding to A in fig. 1) of the first prism, and causing this prism to move so as that the line corresponding to O A F shall be constantly perpendicular to the axis of the incident pencil F G. Since the axis of the incident pencil is fixed in position, this involves only the addition of a fixed slot in the position O A F.

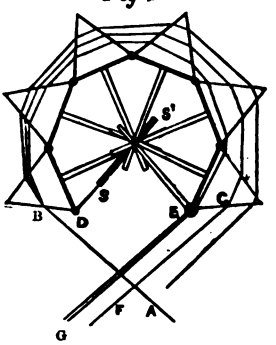
It further occurred to me that since by this arrangement the axis of the emergent pencil from the last prism is situated (as M N in fig. 1) in a direction at right angles to the line from O to the angle corresponding to C, all that is necessary to keep the viewing telescope in its true position, whatever part of the spectrum is studied, is that a slot coincident in direction with this

* Thus, if the constant angle of incidence be ϕ, and the refracting angle of each prism $2i$, minimum deviation is secured only for rays whose refractive index is $\frac{\sin \phi}{\sin i}$.

last-named line should be rigidly attached to an arm bearing the telescope, and should be also at right angles to the optical axis of the telescope.

In fig. 2 the actual position of the prisms is indicated. A B is the axis of the incident pencil at right angles to the fixed slot S S′ (whose direction passes through the angle D of the first prism). The double lines indicate the position of the movable slots, the last of which, O E, is rigidly attached to the viewing telescope, and at right angles to its axis, which corresponds in position with C F, the axis of the emergent pencil.

Fig 2



To obtain the greatest possible dispersion, without losing any part of the spectrum, the refracting angles of the prisms should of course be such that E G will just touch the angle D when the extreme limit of the visible spectrum at its most refrangible end is in the field.

I may add that if A B (fig. 3) be the base of any one of the prisms of the series, it will be evident from the figure that the prism A C B may be replaced by a prism E D F (A E being equal to F B, and E D, F D parallel respectively to A C, B C) without affecting the subsequent path of the *axis* of any pencil; and that for certain purposes the shortening of the path within the prism by such a change would be advantageous. I need not enter into the consideration of the other ways in which a similar shortening can be effected; but I may note that there seems nothing to prevent the equal prisms used in Mr. Browning's present arrangement from being replaced by a series of increasing prisms, represented in section by the shaded triangles in fig. 4 (the last prism of the battery remaining unchanged). If this arrangement is practicably feasible, the definition would, I imagine, be appreciably improved. As the figure shows, the course of the axis of a pencil would not be so altered as to affect the path between the prisms, or the equality of the angles of incidence and emergence.

Fig. 3

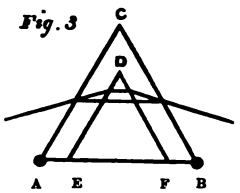
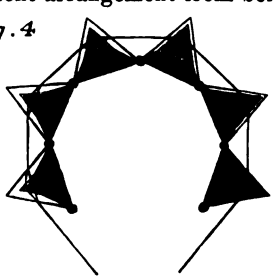


Fig. 4



Since the above was written, I have devised a plan by which

minimum deviation can be secured automatically for a battery of nine, ten, or eleven equilateral prisms. The light from the sixth prism of a series like the above (figs. 2 and 4) is received on a totally reflecting prism, from which it passes to a second battery in a reversed position. By a very simple contrivance the totally reflecting prism adjusts itself, so as always to hold a symmetrical position between the two batteries, the faces including the right-angle of the prism being always perpendicular to the light passing out of one battery and into the other. But as a long connexion, such as this arrangement involves, necessarily works imperfectly (however true theoretically), I at once double the dispersive power and halve the effective length of the connexion, by causing the light to be reflected so as to re-traverse the whole battery, and to be viewed by a fixed telescope whose optical axis is in part coincident with that of the collimator. The motion of adjustment thus has to be communicated to the totally reflecting prism, and the effective length of connexion is the same as for a single battery. I hope to be able to describe this plan in full at the next meeting of this Society. It need hardly be said that the arrangement is only useful where there is abundance of light. One secures by means of it a dispersion corresponding to that given by 22 or 23 equilateral prisms (according to the way in which the light is sent back through the battery); but the power can be reduced prism by prism to any extent which may be requisite.

The Satellites of Uranus. By Richard A. Proctor, B.A.

It has been with extreme regret that I have seen a passage in my *Other Worlds* interpreted by a Vice-President of this Society as a setting up of my mere opinion against the telescopic observations of our President. I hasten to correct this misinterpretation. In expressing the opinion that Sir William Herschel was not deceived as respects the four satellites of *Uranus* which remain unidentified, I was but doing what Admiral Smyth, Professor Nichol, and Professor Grant, had already done (simply on the score of Sir William Herschel's wonderful accuracy as an observer), I am convinced that Mr. Lassell (whose name, by the bye, I had not mentioned in the matter) would be the last to condemn this confidence in the work of an astronomer whose achievements he has so worthily emulated.

I wrote, it is true, with full knowledge that Mr. Lassell has not detected other satellites than *Umbriel*, *Ariel*, *Oberon*, and *Titania*; and with full knowledge also of the qualities of his four-feet mirror. But Sir William Herschel's observations indicate a variability in the lustre of the satellites quite sufficient to account for Mr. Lassell's want of success (so far): and though the light-gathering power of Herschel's four-feet mirror was doubtless inferior to that of Mr. Lassell's, yet it must be remem-

bered that Herschel could not see the satellites when his telescope was used (like Mr. Lassell's) as a Newtonian.*

As Professor Pritchard, at a recent meeting of this Society, spoke of himself as one of scarce three (he thought) present who had read all Sir William Herschel's papers, he will not need to be reminded that Herschel (to use Professor Grant's words) "asserted his firm belief in the existence" of these other satellites. I confess that I should require *very* strong negative evidence to force on me the conviction that Herschel was mistaken on any occasion when he expressed confidence about an observation. Writing with the remembrance of Sir William Herschel's confidence strong upon me, I said in *Other Worlds* that "one cannot read the account of Herschel's method of procedure without feeling that no amount of mere negative evidence can be opposed effectively to the positive information he has left respecting those four orbs." Is this, I would ask, to be considered as an inexcusable attack (as Professor Pritchard has implied) on our esteemed President, who has, indeed, taught us to believe in unseen satellites, ***since his name will for all time be associated with the discovery of an eighth Saturnian satellite long after astronomers had concluded that but seven exist?***

Further Note on the Change of the Colour in Jupiter.

By John Browning.

Since I sent in my Note in reply to the Astronomer Royal's remarks, which appeared in his Report to the Board of Visitors, I have been favoured with a copy of a paper by Prof. A. M. Mayer, of Lehigh University, Pennsylvania, U. S. As this paper appears to me to fully corroborate the substance of the three papers I have had the honour of giving to the Society, I now venture to present some extracts from it:—

"Every astronomer who, during this fall and winter, has made careful observations of *Jupiter*, must have remarked the unusual colours of his disk and belts, and the remarkable forms and mutations which the latter have frequently presented."

* * * * *

"The colour of the planet next demands our attention, and surely no one familiar with the usual tints of the belts and of the general surface can fail to remark the unusual colours which this drawing exhibits.

"*Jupiter's* disk generally is of a light yellow colour crossed by

* Professor Grant thinks it likely that Sir William Herschel refers to his 20 feet mirror, not to the 40-feet one (which was difficult to use) in his account of the discovery of these satellites. But although Sir W. Herschel does not mention specially the 40-feet telescope, it is evident from his remark about the use of the "front-view" (coupled with his original description of that method of using large reflectors) that it was his great mirror he employed. Otherwise, indeed, the whole matter would be infinitely perplexing.

belts of a brownish-grey tint; sometimes, though rarely, approaching a rose colour. Often the brown is entirely wanting, and the belts are merely dull streaks on a light yellow ground.

"We can best convey an accurate conception of the colouring, seen on the 5th of January, by stating the manner in which we tinted the drawing, which, when compared repeatedly at the telescope by myself and others, was found correctly to give the colours and their relative intensities.

"The whole disk was first washed over with a slight tint of yellow. The colour of the region between the equatorial belts was correctly reproduced by pure yellow mixed with crimson-lake, while the two dark belts north, and the one south of the equator were obtained by the same combination of colours, with more crimson-lake added, so as to bring the tint to approach a coppery hue. The polar shadings were correctly given by pure yellow with a slight dash of crimson-lake, which tinting was subsequently overlaid with a very thin wash of light lead colour."

Reading over the last paragraph, one might imagine it to be a description of the last coloured drawing which I have made of *Jupiter*. A copy of this drawing forms the frontispiece to Mr. Proctor's new work, *Other Worlds than Ours*. On referring to this coloured plate, the resemblance may be almost as clearly traced as by examination of the drawing. Professor Mayer used a *refractor* of six inches aperture, made by Alvan Clark.

Clapham, June 30th, 1870.

Further Remarks on the Corona. By Richard A. Proctor, B.A.

At the last meeting of this Society, Dr. Gould, of America, indicated his belief that the trapezoidal corona seen by himself and other observers during the progress of the American eclipse was in fact but the chromosphere seen under unusually favourable circumstances. He added, that the light outside that four-cornered corona appeared to shift in position, and hence he concluded that it was terrestrial.

It seems to me that if this view be admitted, the difficulty pointed out by Mr. Lockyer in the case of the corona considered generally, exists in scarcely diminished extent in the case of this trapezoidal appendage also. Estimated by most of the observers as extending fully 12′ from the disc of the eclipsed Sun, its real depth would be far more than 320,000* miles, and the pressure even at the summits of the highest prominences would be enormous.

We gain nothing, then, by Dr. Gould's supposition; though of course that does not prove it to be erroneous. But Dr.

* Even in Mr. Whipple's photograph it has an extent of fully 6′, which would correspond to more than 160,000 miles, or 80,000 miles above the highest prominences yet seen.

Curtis (whose successful photographs appear in Commodore Sands's reports of the total eclipse) remarks that he has read Dr. Gould's statements respecting the eclipse with considerable surprise. After referring to the photographic evidence, he adds, "Dr. Gould adduces as an additional argument in favour of his assumption the observation that the long coronal beams appeared to him to be 'variable,' while the 'aureole' photographed was evidently 'constant' during the time of totality. This argument, however, loses some of its force, when it is remembered that to other observers the corona appeared to the eye absolutely unchangeable, both in form and position, during the whole period of the total obscuration." He goes on to indicate the probability that Dr. Gould has mistaken a photographic effect for a real phenomenon, in this case, precisely as when he interpreted the apparent encroachment of the bases of the prominences on the Moon (a dark-room phenomenon, as Curtis shows) to "specular reflection" at the Moon's surface.

I must confess, that after a very careful study of the whole series of American observations, Dr. Gould's view appears to me to be altogether disposed of by the concurrent testimony of so many and such skilful observers.

One striking, and as yet unnoticed, piece of evidence exists in General Myers' report of the appearance of the corona as seen from the summit of White Top Mountain, 5530 feet above the sea-level. Here the same quadrangular aspect was observed as at lower levels (and in Whipple's photograph), but the rays were much longer. "The silvery rays," he says, "were longest and most prominent at four points of the circumference—two upon the upper, and two upon the lower portion—apparently equidistant from each other, and at about the junctions of the quadrants designated as 'limbs,' giving the spectacle a quadrilateral shape." He remarks that these silvery rays were "straight and massive," and extended "to a distance of two or three diameters of the lunar disc." He adds, "There was no motion of the rays."

It seems impossible to mistake the significance of these observations.

In my paper in the March number of our *Notices*, I dealt specially with the theory that the corona is due to the illumination of the Earth's atmosphere by light not affected "by any action at the Moon." Many of the arguments, however, apply equally well on the supposition that there is such action. The striking fact that at the time of central eclipse the cone within our atmosphere bounded by lines from the observer's eye to the Moon's limb, contains no light, while the cylinder within our atmosphere bounded by lines from the Sun's limb to (and produced beyond) the Moon's, contains much light, affords, I take it, absolutely convincing evidence that this light is derived from an object far beyond the Moon. For if we suppose the solar rays to get by any process within the cylinder, they should

clearly traverse the cone also. For example, assuming that a solar ray passing by the Moon's edge is deflected (by whatever cause) so as to fall within that cylinder into which (from its very nature) undeflected rays cannot pass, the deflection, in order to account for observed appearances, must carry the illumination of our atmosphere up to the above-mentioned cone, and there suddenly the illumination must cease. But the cone has no existence in nature; it is but a mathematical conception: why then should these deflected rays respect it?*

Even La Hire's theory, which De Lisle is supposed to have overthrown, seems more easily supported than one which requires a moving shadow-cylinder of air to be illuminated, while a fixed cone (*not* a shadow cone) within it remains in darkness.

It seems much more natural to regard the blackness of the lunar disc, and the relative brightness of the corona, as due simply to the fact that the Moon is an opaque body very much nearer to us than the corona.

Let me renew my statement that it is the importance of the approaching eclipse which forces me to urge now views which I have long entertained. It appears to me that if, as I hold to be the case, the evidence respecting the corona is amply sufficient to prove it to be a solar appendage, then it would be a serious misfortune if any observers were to devote their time to establishing this fact. Instead of this, I should be glad to see every moment of the short duration of totality devoted both by general observers and spectroscopists to the inquiry *what sort of a solar appendage* the corona may be. On this inquiry depend issues of the utmost interest and importance to science; the other would be a waste of time: on one question we have abundant evidence; on the other (to quote the just words† of Professor Pritchard), "wise astronomers profess their profound ignorance."

* Mr. Lockyer tells me that M. Faye expressly suggests that there is some action at the Moon, and that, according to his and M. Faye's theory, it is thus the atmosphere gets illuminated. What the nature of the action may be I have not yet heard, nor can I conceive of any which would account, however roughly, for observed facts. Mr. Seabroke's paper on the Corona in the last number of our *Notices*, assumes no such action to take place; yet it is supposed that he is there defending Mr. Lockyer's theory. On the other hand, let me note in passing, Mr. Seabroke deals with an imaginary (I had almost said impossible) eclipse, and is therefore evidently not attacking my views respecting real eclipses. His arguments are, however, mathematically accurate. At the very moment and place of second and third contacts in an ordinary total eclipse, or during the occurrence of an exact total eclipse (which could last of course but an instant), the results he educes would doubtless take place, though it would be wholly impossible to observe them. But his formulæ are wholly inapplicable to the circumstances of any actual eclipse.

† Just *per se*, though somewhat too magisterially applied as a warning to myself. I also profess complete ignorance as to the nature and condition of the material forming the corona, but it is on account of that ignorance that I am so anxious to see the skilled observers of the approaching eclipse employing the opportunity in the most advantageous manner.

The use of the Steel-Balance in estimating the Distribution of Stars and Nebulæ. By Richard A. Proctor, B.A.

It is often difficult to estimate the extent of any irregular district of the heavens, and hence the numerical statistics of star-distribution require to be supplemented by a means enabling us to deal readily with spherical areas. I propose for this purpose (and have already begun to apply) the following method. Having laid down any convenient isographic projection, let the boundaries of any space whose area we require be traced on that projection. Then let the included space be carefully cut out and weighed against the remainder of the projection, with any convenient form of the steel-balance. The area of the space thus becomes known, and the relative richess of star-distribution can at once be determined.

Suppose, for example, a certain region r contains n stars of a given order (or nebulæ, as the case may be), while the whole heavens (R say) contain N such objects. Suppose by means of the steel-balance we find the following relation between the weights w and $(W - w)$ of the regions r and $(R - r)$:

$$\frac{w}{W-w} = \frac{1}{a}\text{; then } \frac{w}{W} = \frac{1}{a+1}$$

and the diversity of distribution in the region r is to the average density of the whole heavens as $\frac{n}{1} : \frac{N}{a+1}$, or as $(a+1)\ n : N$.

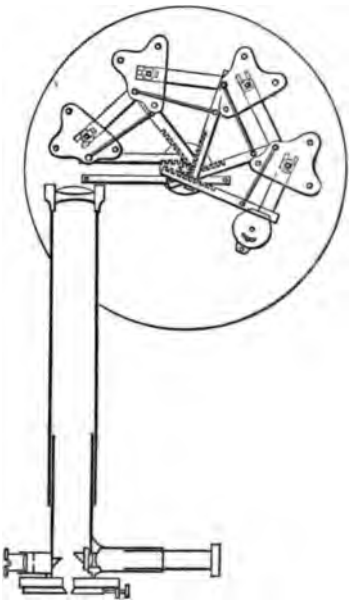
The construction of a polar isographic projection by the method described in my *Handbook of the Stars* (originally designed, I afterwards learned, by Sir John Herschel) is so simple a matter, and can be effected so accurately, that the plan described above affords a ready means of solving a number of problems connected with the areas of spherical surfaces. For example, I believe a more accurate estimate of the relation between the land and sea surfaces of our Earth could be obtained in this way than by the methods hitherto applied.

It is obviously quite as easy to compare any two celestial regions together by this method as to compare a given region with the whole heavens.

I hope at the next meeting of the Society to exhibit some results obtained by this method, which seem to indicate a surprising inequality in the distribution of stars in different portions of the heavens—I mean among those orders of magnitude within which, if the views at present accepted were true, a general equality was to have been expected according to the laws of probability.

Reply to M. de Littrow's remarks on the Automatic Spectroscope. By John Browning.

Having obtained a copy of the Number of the *Proceedings of the Imperial Academy of Sciences of Vienna*, vol. xlvii. which contains a description, with figures, of Herr von Littrow's Spectroscope, I am enabled to present the readers of the *Monthly Notices* with a copy of the principal diagram. On comparing this with the diagram of the instrument which I have contrived and described in *Monthly Notices*, vol. xxx. p. 198, I do not think any one will agree that, "the only difference between the two instruments consists in the sparing of the second telescope."



If I understand Herr Von Littrow's plan he has devised a symmetrical arrangement of the prisms, and communicated motion to each prism separately by means of a rack on a rod attached to the base of each prism, at right angles to the base, controlling this motion by means of cords. The adjustment of such an instrument must be necessarily tentative—each prism being brought right separately in the first instance; and it would be very difficult to so adjust it, that by a continuation of the rack-work motions, each prism should move with the requisite variable motion when the apparatus is put in action.

In the plan I have contrived, the prisms are so arranged that as the rod from the base of each can move freely over a common centre, and the whole of the prisms are linked together, a motion given to any part of the train of prisms must produce the desired effect, because the bases of the prisms remain always at a tangent to an inscribed circle, and, consequently, the angles between all the prisms remain the same.

I must disclaim having had any previous knowledge of Herr von Littrow's invention.

ERRATA

In Monthly Notices for June 10, 1870.

Vol. xxx. No. 8.

Page 203, line 20 from bottom, *for* Canis Venatica, *read* Canes Venatici.

— — No. of Observations of N.P.D. in 7-year Cat. (1860), *for* 8, *read* 18.

— 204, N.P.D. of Groombridge, 1830, for 1869,
for 51° 20′ 29″·84, *read* 51° 20′ 29″·98.
Annual Variation in N.P.D. for years 1864—1869,
for + 25″·72, *read* + 25″·75.
Proper Motion in N.P.D. for years 1864—1869,
for + 5″·71, *read* + 5″·74.

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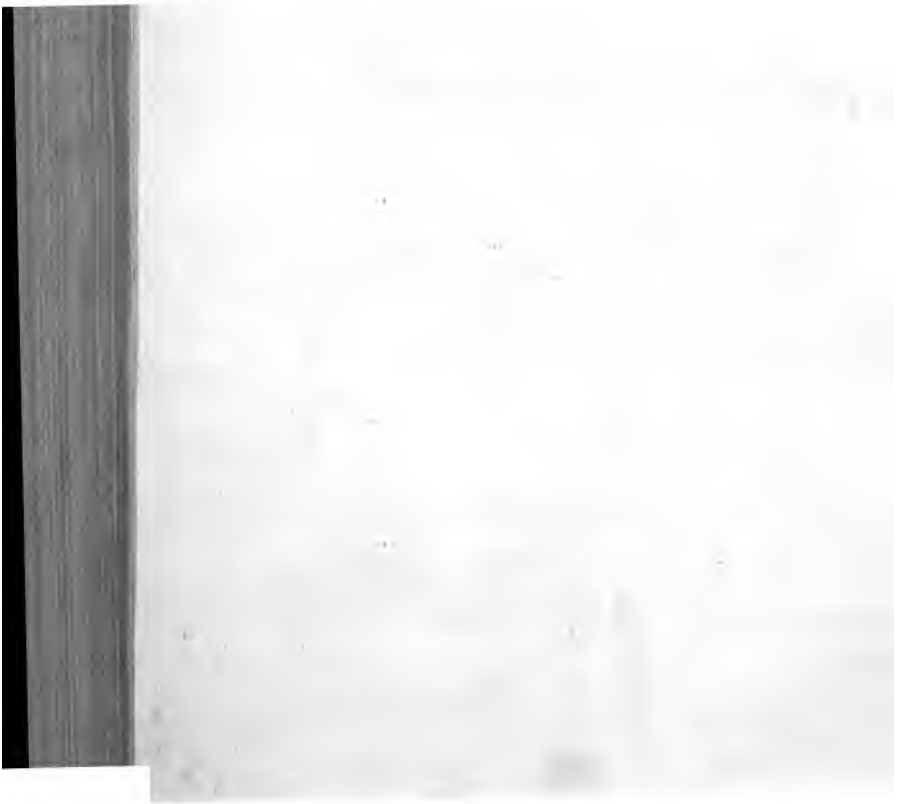
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